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OF
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JANUARY TO JUNE, 1822.

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ERRATA.

Page 16, *dele* Theory of Evaporation.
line 3, Case 1, *for* Q, q, t, *read* Q, q, t'.
21, Art. 16, *for* 32°, *read* the zero of.
22, 26, Confirmation, *for* thai, *read* that.
23, 33, *for* expand, *read* expand most.
26, 76, *for* 275, *read* 375.
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89, *after* Calculation, *read* page 448.
for 116, *read* ·116.
28, line 2, from top, *after* Annals of Philosophy, *read* New Series.
31, line 7, *for* and, *read* or
85, line 14, from bottom, *for* 13, *read* 18.
86, line 28, *for* 1·25, *read* ·125. This occasions an unimportant error in the statement of the analysis.

ANNALS

OF

PHILOSOPHY.

JANUARY, 1822.

Article I.

*Further Researches on the Magnetic Phænomena produced by Electricity; with some new Experiments on the Properties of Electrified Bodies in their Relations to conducting Powers and Temperature.** By Sir Humphry Davy, Bart. PRS.

I. In my letter to Dr. Wollaston on the new facts discovered by M. Oersted, which the Society has done me the honour to publish, I mentioned, that I was not able to render a bar of steel magnetic by transmitting the electrical discharge across it through a tube filled with sulphuric acid; and I have likewise mentioned, that the electrical discharge passed across a piece of steel through air, rendered it less magnetic than when passed through a metallic wire; and I attributed the first circumstance to the sulphuric acid being too bad a conductor to transmit a sufficient quantity of electricity for the effect; and the second, to the electricity passing through air in a more diffused state than through metals.

To gain some distinct knowledge on the relations of the different conductors to the magnetism produced by electricity, I instituted a series of experiments, which led to very decisive results, and confirmed my first views.

II. I found that the magnetic phænomena were precisely the same, whether the electricity was small in quantity, and passing through good conductors of considerable magnitude; or, whether the conductors were so imperfect as to convey only a small quantity of electricity; and in both cases they were neither attractive of each other, nor of iron filings, and not affected by the magnet; and the only proof of their being magnetic, was their occasioning a certain small deviation of the magnetized needle.

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* From the Philosophical Transactions, for 1821, Part II.

Thus, a large piece of charcoal placed in the circuit of a very powerful battery, being a very bad conductor compared with the metals, would not affect the compass needle at all, unless it had a very large contact with the metallic part of the circuit; and if a small wire was made to touch it in the circuit only in a few points, that wire did not gain the power of attracting iron filings; though, when it was made to touch a surface of platinum foil coiled round the end of the charcoal, a slight effect of this kind was produced. And in a similar manner fused hydrate of potassa, one of the best of the imperfect conductors, could never be made to exert any attractive force on iron filings, nor could the smallest filaments of cotton moistened by solution of hydrate of potassa, placed in the circuit, be made to move by the magnet; nor did steel needles floating on cork on an electrized solution of this kind, placed in the voltaic circuit, gain any polarity; and the only proof of the magnetic powers of electricity passing through such a fluid, was afforded by its effect upon the magnetized needle, when the metallic surfaces, plunged in the fluid, were of considerable extent. That the mobility of the parts of fluids did not interfere with their magnetic powers as developed by electricity, I proved, by electrifying mercury, and Newton's metal fused, in small tubes. These tubes, placed in a proper voltaic circuit, attracted iron filings, and gave magnetic powers to needles; nor did any agitation of the mercury or metal within, either in consequence of mechanical motion or heat, alter or suspend their polarity.

III. Imperfect conducting fluids do not give polarity to steel when electricity is passed through them; but electricity passed through air produces this effect. Reasoning on this phænomenon, and on the extreme mobility of the particles of air, I concluded, as Mr. Arago had likewise done from other considerations, that the voltaic current in air would be affected by the magnet. I failed in my first trial, which I have referred to in a note to my former paper, and in other trials made since by using too weak a magnet; but I have lately had complete success; and the experiment exhibits a very striking phænomenon.

Mr. Pepys having had the goodness to charge the great battery of the London Institution, consisting of 2000 double plates of zinc and copper, with a mixture of 1168 parts of water, 108 parts of nitrous acid, and 25 parts of sulphuric acid, the poles were connected by charcoal, so as to make an arc, or column of electrical light, varying in length from one to four inches, according to the state of rarefaction of the atmosphere in which it was produced; and a powerful magnet being presented to this arc or column, having its pole at a very acute angle to it, the arc, or column, was attracted or repelled with a rotatory motion, or made to revolve, by placing the poles in different positions, according to the same law as the electrified cylinders of platinum described in my last paper, being repelled when the negative pole was on the right hand by the north pole of the magnet, and attracted by the south pole, and *vice versâ*.

It was proved by several experiments that the motion depended entirely upon the magnetism, and not upon the electrical inductive power of the magnet, for masses of soft iron, or of other metals, produced no effect.

The electrical arc or column of flame was more easily affected by the magnet, and its motion was more rapid when it passed through dense than through rarified air; and in this case, the conducting medium or chain of aëriform particles was much shorter.

I tried to gain similar results with currents of common electricity sent through flame, and in vacuo. They were always affected by the magnet; but it was not possible to obtain so decided a result as with voltaic electricity, because the magnet itself became electrical by induction, and that whether it was insulated, or connected with the ground.*

IV. Metals, it is well known, readily transmit large quantities of electricity; and the obvious limit to the quantity which they are capable of transmitting seems to be their fusibility, or volatilization by the heat which electricity produces in its passage through bodies.

Now I had found in several experiments, that the intensity of this heat was connected with the nature of the medium by which the body was surrounded; thus a wire of platinum which was readily fused by transmitting the charge from a voltaic battery in the exhausted receiver of an air-pump, acquired in air a much lower degree of temperature. Reasoning on this circumstance, it occurred to me, that by placing wires in a medium much denser than air, such as ether, alcohol, oils, or water, I might enable them to transmit a much higher charge of electricity than they could convey without being destroyed in air; and thus not only gain some new results as to the magnetic states of such wires, but likewise, perhaps, determine the actual limits to the powers of different bodies to conduct electricity, and the relations of these powers.

A wire of platinum of $\frac{1}{220}$, of three inches in length, was fused in air, by being made to transmit the electricity of two batteries of ten zinc plates of four inches with double copper, strongly charged: a similar wire was placed in sulphuric ether, and the charge transmitted through it. It became surrounded by globules of gas; but no other change took place; and in this situation it bore the discharge from twelve batteries of the same kind, exhibiting the same phænomena. When only about an inch of it was heated by this high power in ether, it made the ether boil, and became white hot under the globules of vapour, and then rapidly decomposed the ether, but it did not fuse. When oil or water was substituted for the ether, the length of the wire remaining the same, it was partially covered with small globules of gas, but did not become red hot.

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* I made several experiments on the effects of currents of electricity simultaneously passing through air in different states of rarefaction in the same and different directions, both from the voltaic and common electrical batteries; but I could not establish the fact of their magnetic attractions or repulsions with regard to each other, which probably was owing to the impossibility of bringing them sufficiently near.

On trying the magnetic powers of this wire in water, they were found to be very great, and the quantity of iron filings that it attracted was such as to form a cylinder round it, of nearly the tenth of an inch in diameter.

To ascertain whether short lengths of fine wire, prevented from fusing by being kept cool, transmitted the whole electricity of powerful voltaic batteries, I made a second independent circuit from the ends of the battery with silver wires in water, so that the chemical decomposition of the water indicated a residuum of electricity in the battery. Operating in this way, I found that an inch of wire of platinum of $\frac{1}{220}$, kept cool by water, left a great residual charge of electricity in a combination of twelve batteries of the same kind as those abovementioned; and after making several trials, I found that it was barely adequate to discharge six batteries.

V. Having determined that there was a *limit* to the quantity of electricity which wires were capable of transmitting, it became easy to institute experiments on the different conducting powers of different metallic substances, and on the relation of this power to the temperature, mass, surface, or length, of the conducting body, and to the conditions of electro-magnetic action.

These experiments were made as nearly as possible under the same circumstances, the same connecting copper wires being used in all cases, their diameter being more than one-tenth of an inch, and the contact being always preserved perfect; and parts of the same solutions of acid and water were employed in the different batteries, and the same silver wires and broken circuit with water were employed in the different trials; and when no globules of gas were observed upon the negative silver wire of the second circuit, it was concluded that the metallic conducting chain, or the primary circuit, was adequate to the discharge of the combination. To describe more minutely all the precautions observed, would be tedious to those persons who are accustomed to experiments with the voltaic apparatus, and unintelligible to others; and after all, in researches of this nature, it is impossible to gain more than approximations to true results; for the gas disengaged upon the plates, the different distances of the connecting plates, and the slight difference of time in making the connections, all interfere with their perfect accuracy.

The most remarkable general result that I obtained by these researches, and which I shall mention first, as it influences all the others, was, that *the conducting power of metallic bodies varied with the temperature, and was lower in some inverse ratio as the temperature was higher.*

Thus a wire of platinum of $\frac{1}{220}$, and three inches in length, when kept cool by oil, discharged the electricity of two batteries, or of 20 double plates; but when suffered to be heated by exposure in the air, it barely discharged one battery.

Whether the heat was occasioned by the electricity, or applied to it from some other source, the effect was the same. Thus a wire of platinum, of such length and diameter as to discharge a combination without being considerably heated; when the flame of a spirit lamp was applied to it so as to make a part of it red hot, lost its power of discharging the whole electricity of the battery, as was shown by the disengagement of abundance of gas in the secondary circuit; which disengagement ceased as soon as the source of heat was withdrawn.

There are several modes of exhibiting this fact, so as to produce effects which, till they are witnessed, must almost appear impossible. Thus, let a fine wire of platinum of four or five inches in length be placed in a voltaic circuit, so that the electricity passing through it may heat the whole of it to redness, and let the flame of a spirit lamp be applied to any part of it, so as to heat that part to whiteness, the rest of the wire will instantly become cooled below the point of visible ignition. For the converse of the experiment, let a piece of ice or a stream of cold air be applied to a part of the wire; the other parts will immediately become much hotter; and from a red, will rise to a white heat. The quantity of electricity that can pass through that part of the wire submitted to the changes of temperature is so much smaller when it is hot than when it is cold, that the absolute temperature of the whole wire is diminished by heating a part of it, and, *vice versâ*, increased by cooling a part of it.

In comparing the conducting powers of different metals, I found much greater differences than I had expected. Thus six inches of silver wire of $\frac{1}{220}$ discharged the whole of the electricity of 65 pair of plates of zinc and double copper made active by a mixture of about one part of nitric acid of commerce, and 15 parts of water. Six inches of copper wire of the same diameter discharged the electricity of 56 pairs of the same combination, six inches of tin of the same diameter carried off that of 12 only, the same quantity of wire of platinum that of 11, and of iron that of 9. Six inches of wire of lead of $\frac{1}{200}$ seemed equal in their conducting powers to the same length of copper wire of

$\frac{1}{220}$. All the wires were kept as cool as possible by immersion in a basin of water.*

I made a number of experiments of the same kind, but the results were never precisely alike, though they sometimes approached very near each other. When the batteries were highly charged, so that the intensity of the electricity was higher, the differences were less between the best and worst conductors, and they were greater when the charge was extremely feeble. Thus, with a fresh charge of about one part of nitric acid, and nine parts of water, wires of $\frac{1}{220}$ of silver and platinum five inches long, discharged respectively the electricity of 30, and seven double plates.

Finding that when different portions of the same wire plunged in a non-conducting fluid were connected with different parts of the same battery equally charged, their conducting powers appeared in the inverse ratio of their lengths; so, when six inches of wire of platinum of $\frac{1}{220}$ discharged the electricity of 10 double plates, 3 inches discharged that of 20, $1\frac{1}{2}$ inch that of 40, and 1 inch that of 60; it occurred to me that the conducting powers of the different metals might be more easily compared in this way, as it would be possible to make the contacts in less time than when the batteries were changed, and consequently with less variation in the charge.

Operating in this way, I ascertained that in discharging the electricity of 60 pairs of plates, 1 inch of platinum was equal to about 6 inches of silver, to $5\frac{1}{2}$ inches of copper, to 4 of gold, to 3·8 of lead, to about $\frac{9}{10}$ of palladium, and $\frac{8}{10}$ of iron, all the metals being in a cooling fluid medium.

I found, as might have been expected, that the conducting power of a wire for electricity, in batteries of the size and number of plates just described, was nearly directly as the mass; thus, when a certain length of wire of platinum discharged one battery,† the same length of wire of six times the weight discharged six batteries; and the effect was exactly the same, provided the wires were kept cool, whether the mass was a single wire, or composed of six of the smaller wires in contact with each other. This result alone showed, that surface had no relation to conducting power, at least for electricity of this kind, and it was more distinctly proved by a direct experiment; equal lengths and equal weights of wire of platinum, one round, and one flattened by being passed transversely through rollers so as to have six or seven times the surface, were compared as to conducting powers: the flattened wire was the best conductor in air from its greater cooling powers, but in water no difference could be perceived between them.

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* Water is so bad a conductor, that in experiments of this kind, its effects may be neglected altogether; and these effects were equal in all the experiments.

† A foot of this wire weighed 1·13 grains; a foot of the other, 6·7 grains.

VI. I tried to make a comparison between the conducting powers of fluid menstrua and charcoal and those of metals. Six inches of platinum foil, an inch and one-fifth broad, were placed in a vessel which could be filled with any saline solution; and a similar piece of platinum placed opposite at an inch distance; the whole was then made part of a voltaic circuit, which had likewise another termination by silver wires in water; and solution of salts added, till gas ceased to be liberated from the negative silver wire. In several trials of this kind, it was found that the whole of the surface of six inches, even with the strongest solutions of common salt, was insufficient to carry off the electricity even of two pair of plates; and a strong solution of potassa carried off the electricity of three pair of plates only; whereas an inch of wire of platinum of $\frac{1}{220}$ (as has been stated) carried off all the electricity of 60 pair of plates. The gas liberated upon the surface of the metals when they are placed in fluids, renders it impossible to gain accurate results; but the conducting power of the best fluid conductors, it seems probable from these experiments, must be some hundreds of thousand times less than those of the worst metallic conductors.

A piece of well-burnt compact box-wood charcoal was placed in the circuit, being $\frac{3}{10}$ of an inch wide by $\frac{1}{10}$ thick, and connected with large surfaces of platinum. It was found that 1 inch and $\frac{3}{10}$ carried off the same quantity of electricity as 6 inches of wire of platinum of $\frac{1}{220}$.

VII. I made some experiments with the hope of ascertaining the exact change of ratio of the conducting powers dependent upon the change of the intensity and quantity of electricity; but I did not succeed in gaining any other than the general result, that the higher the intensity of the electricity, the less difficulty it had in passing through bad conductors; and several remarkable phænomena depend upon this circumstance.

Thus, in a battery where the quantity of the electricity is very great, and the intensity very low, such as one composed of plates of zinc and copper, so arranged as to act only as single plates of from 20 to 30 feet of surface each, and charged by a weak mixture of acid and water. Charcoal made to touch only in a few points is almost as much an insulating body as water, and cannot be ignited, nor can wires of platinum be heated when their diameter is less than $\frac{1}{80}$ of an inch, and their length three or four feet; and a foot of platinum wire of $\frac{1}{30}$ is scarcely heated by such a battery, whilst the same length of silver wire of the same diameter is made red hot; and the same lengths of thicker wires of platinum or iron are intensely heated.

The heat produced where electricity of considerable intensity is passed through conductors, must always interfere with the exact knowledge of the changes of their conducting powers, as is proved by the following experiment: A battery of 20 pair of plates of zinc, and copper plates 10 inches by 6, was very highly charged with a mixture of nitric acid and water, so as to exhibit a considerable intensity of electrical action, and the relative conducting powers of silver and platinum in air and water ascertained by means of it. In air, 6 inches of wire of platinum of $\frac{1}{80}$, discharged only 4 double plates, whilst 6 inches of silver wire of the same diameter discharged the whole combination: the platinum was strongly ignited in this experiment, whilst the silver was scarcely warm to the touch. On cooling the platinum wire by placing it in water, it was found to discharge 10 double plates. When the intensity of the electricity is very high, however, even the cooling powers of fluid media are of little avail: thus I found that fine wire of platinum was fused by the discharge of a common electrical battery under water; so that the conducting power must always be diminished by the heat generated, in a greater proportion as the intensity of the electricity is higher.

It might, at first view, be supposed, that when a conductor placed in the circuit left a residuum of electricity in any battery, increase of the power of the battery, or of its surface, would not enable it to carry through any additional quantity. This, however, is far from being the case.

When saline solutions were placed in the circuit of a battery of 20 plates, though they discharged a very small quantity only of the electricity, when the troughs were only one quarter full, yet their chemical decomposition exhibited the fact of a much larger quantity passing through them, when the cells were filled with fluid.

And a similar circumstance occurred with respect to a wire of platinum, of such a length as to leave a considerable residuum in a battery when only half its surface was used; yet when the whole surface was employed, it became much hotter, and nevertheless left a still more considerable residuum.

VIII. I found long ago, that in increasing the number of alternations of similar plates, the quantity of electricity seemed to increase as the number, at least as far as it could be judged of by the effects of heat upon wires; but only within certain limits, beyond which the number appeared to diminish, rather than increase the quantity. Thus the 2000 double plates of the London Institution, when arranged as one battery, would not ignite so much wire as a single battery of 10 plates with double copper.

It is not easy to explain this result. Does the intensity mark the rapidity of the motion of the electricity? or merely its diminished attraction for the matter on which it acts? and does this attraction become less in proportion as the circuit through which it passes, or in which it is generated, contains a greater number of alternations of bad conductors?

Mr. Children, in his account of the experiments made with his battery of large plates, has ingeniously referred the heat produced by the passage of electricity through conductors, to the resistance it meets with, and has supposed, what proves to be the fact, that the heat is in some inverse ratio to the conducting power. The greatest heat, however, is produced in air, where there is reason to suppose the least resistance; and as the presence of heat renders bodies worse conductors, another view may be taken; namely, that the excitation of heat occasions the imperfection of the conducting power. But till the causes of heat and of electricity are known, and of that peculiar constitution of matter which excites the one, and transmits or propagates the other, our reasoning on this subject must be inconclusive.

I found that when equal portions of wires of the same diameter, but of different metals, were connected together in the circuit of a powerful voltaic battery, acting as two surfaces, the metals were heated in the following order: iron most, then palladium, then platinum, then tin, then zinc, then gold, then lead, then copper, and silver least of all. And from one experiment, in which similar wires of platinum and silver joined in the same circuit were placed in equal portions of oil, it appeared that the generation of heat was nearly inversely as their conducting power. Thus the silver raised the temperature of the oil only four degrees, whilst the platinum raised it 22. The same relations to heat seem to exist, whatever is the intensity of the electricity; thus circuits of wires placed under water, and acted on by the common electrical discharge, were heated in the same order as by the voltaic battery, as was shown by their relative fusion; thus, iron fusing before platinum, platinum before gold, and so on.

If a chain be made of wire of platinum and silver, in alternate links soldered together, the silver wire being four or five times the diameter of the platinum, and placed in a powerful voltaic circuit, the silver links are not sensibly heated, whilst all those of the platinum become intensely and equally ignited. This is an important experiment for investigating the nature of *heat*. If heat be supposed a substance, it cannot be imagined to be expelled from the platinum; because an unlimited quantity may be generated from the same platinum, *i.e.* as long as the electricity is excited, or as often as it is renewed. Or if it be supposed to be identical with, or an element of, electricity, it ought to bear some relation to its quantity, and might be expected to be the same in *every* part of the chain, or greatest in those parts nearest the battery.

IX. The magnetism produced by electricity, though with the same conductors it increases with the heat, as I mentioned in my last paper; yet with different conductors I find it follows a very different law. Thus, when a chain is made of different conducting wires, and they are placed in the same circuit, they all exhibit equal magnetic powers, and take up equal quantities of iron filings. So that the magnetism seems directly as the quantity of electricity which they transmit. And when in a highly powerful voltaic battery, wires of the same diameters and lengths, but of which the best conducting is incapable of wholly discharging the battery, are made, separately and successively, to form the circuit, they take up different quantities of iron filings, in some direct proportion to their conducting powers.

Thus, in one experiment, two inches of wire of $\frac{1}{30}$ of an inch being used, silver took up 32 grains, copper 24, platinum 11, and iron 8$\frac{2}{10}$.

Article II.

On a new Anemometer. By Col. Beaufoy, FRS.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR, *Bushey Heath, Dec.* 14, 1821.

In the last number of the *Annals*, you did me the favour of inserting the description and engraving of a new anemometer, which, it has been suggested, would be rendered more complete, by the accompanying tables of the impulse of the wind.

I have, therefore, the pleasure to forward the annexed.

I remain, dear Sir, truly yours,

Mark Beaufoy.

ANEMOMETRICAL TABLE.

Weights suspended to the Cord G.

1 Ounce.			4 Ounces.			1 lb.			4 lbs.		
	lbs.	Diff.		lbs.	Diff.		lbs.	Diff.		lbs.	Diff.
1	0·1972		1	0·4147		1	1·3707		1	5·1280	
2	0·2313	0·0341	2	0·5440	0·1293	2	1·8932	0·5225	2	7·2858	2·1578
3	0·2654	0·0341	3	0·6732	0·1292	3	2·4165	0·5233	3	9·4435	2·1577
4	0·2996	0·0342	4	0·8025	0·1293	4	2·9393	0·5228	4	11·6012	2·1577
5	0·3337	0·0341	5	0·9318	0·1293	5	3·4623	0·5230	5	13·7590	2·1578
1	2	3	1	2	3	1	2	3	1	2	3

Column 1, contains the revolutions of the fusee. Column 2, the value of each revolution in pounds avoirdupoise, and decimal parts of a pound. Column 3, the differences, one-tenth of which is the value of ten divisions, and these last numbers being divided by 10, give the value of one division.

Example 1.—Suppose when four pounds are hung to the cord

G, and that the action of the wind moved the hands four turns and 76 divisions.

4 turns .... = 11·6012
2·1578 × 76. .... = 1·6399

Momentum in lbs. .... 13·2411

Example 2.—Suppose one ounce suspended to the cord, and that the wind moved the hand 60 divisions. 100 — 60 = 40; ·0341 × ·40 = ·0136.

1 turn .... = 0·1972
Subtract .... 0·0136

Momentum .... 0·1836 lbs.

ARTICLE III.

Chemical Examination of Spiders' Web.

(To the Editor of the *Annals of Philosophy*.)

RESPECTED FRIEND, *Manchester, Eleventh Month*, 20, 1821.

HAPPENING a short time ago to apply the flame of a candle to a collection of spider's web, my attention was excited by the formation of a large quantity of dense white vapours which ascended during the combustion. It struck me that the phenomenon was probably occasioned by a volatile salt.

The following particulars are all which I have yet ascertained: I purpose to resume the inquiry; and if any facts of interest should present themselves, I shall again trouble thee with a communication on the subject. I find that all webs have not precisely the same properties. Those specimens which I procured from a moist dark vault, differ from others obtained from lighter and more airy situations. The former, by the addition of cold water, afford a deep-brown coloured solution; while the latter gave only a tint of yellow. By an application of the necessary tests, I find that the webs which I have examined contain lime, together with muriatic and sulphuric acids. A part of the first of the cold water solutions, just mentioned, was boiled in a flask; when the water had evaporated, the residuum emitted a very offensive smell, resembling that of animal matter when cast upon hot iron. When the heat was continued, there arose copious white fumes. The matter remaining, after the last operation, was of a brown colour. To this matter, when the vessel had cooled, a quantity of fresh water was added; the solution was then allowed to boil for a few minutes. The water had by this means no colour imparted to it; but, when tested with hydrochlorate of baryta, a precipitate was instantly occasioned. Oxalate of ammonia produced a precipitate; but nitrate of silver had not the least effect. Some of the first mentioned residuum, after having been well washed, was heated to redness in a silver spoon; there remained a greyish coloured earth, but it was not in sufficient quantity to enable me to ascertain its properties. It is not improbable, I imagine, that the white fumes were occasioned by muriate of ammonia; and this conjecture is countenanced by the circumstance, that if water be added to the residuum, no precipitate is produced by nitrate of silver.

An accurate analysis of the spider's-web might lead to some results interesting alike to the chemist, and to the student of natural history.

I remain, thine very truly, O. S.

P.S. I might also add, that subsequently to writing the preceding statement, I made a quantity of the web into a paste with warm water; I then mixed it with a paste formed of warm water and hydrate of lime, when I found an immediate evolution of ammoniacal gas, confirming my conjecture as to the presence of muriate of ammonia.

Article IV.

Rules and Examples for the perpetual Renewal of Leases.

By Mr. James Adams.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, near Plymouth, Oct.* 11, 1821.

Having often had occasion to calculate the value of leasehold property held on perpetual renewals, I deduced the following particular rules from the more general ones given by the writers on Life Annuities, and having found them useful to myself, I judge that they may be useful to others who have similar calculations to make. Your inserting them with the following examples, &c. in the *Annals of Philosophy*, will oblige

Your humble servant,

James Adams.

◆

A lord of a manor grants to a tenant a piece of land on condition that he builds on a certain part of it a substantial dwelling house and appendages, for which the tenant is to have a lease on three of the best lives he can find, and that he and his successors shall be allowed to fill up the lease continually whenever a life may drop, by paying a proposed *fine* and an *annual* conventionary rent, to find the landlord's interest in the lease.

Rule.—Subtract the value of the best life (taken from the most approved tables) from the perpetuity; then it will be

As the value of the best life
Is to the above difference,
So is three times the fine for each renewal
To the present value of all the sums paid for renewing.

Which, added to the yearly conventionary rent multiplied by the perpetuity, will give the present value of the landlord's interest in the lease.

When the interest is either 4 or 5 per cent. the numbers in the following table multiplied by the fine for renewal, and the product added to the yearly conventionary rent multiplied by the perpetuity, will give the landlord's interest as above:

Where the tables are kept.	Numbers corresponding to 4 per cent.	Numbers corresponding to 5 per cent.
Carlisle	0·78936	0·57355
France	0·94280	0·70073
Sweden	1·00534	0·74298
Northampton	1·24640	0·94063
Demoivre	1·44260	1·10762
London	1·57317	1·19580

Example 1.—A lessor grants a lease of a house to a lessee on three of the best lives that can be found, with a perpetual right of renewal, subject to an annual conventionary rent of 30*s.* an heriot on the death of each life 3*l.* and the fine for renewal 30*l.* required the present value of the lessor's interest in the house, interest being 3½ per cent.

Answer.—The value of the best life, from observations made in France, according to M. de Parceiux, is 20·77, the value of the perpetuity $\frac{200}{7} = 28{\cdot}57143$, and $28{\cdot}57143 - 20{\cdot}77 = 7{\cdot}80143$; then by the rule we have, $20{\cdot}77 : 7{\cdot}80143 :: 3\,(3 + 30) : 37{\cdot}185$, the present value of all the heriots and fines, to which add $28{\cdot}57143 \times 1{\cdot}5 = 42{\cdot}857$, and the sum $80{\cdot}042 =$ 80*l.* 0*s.* 10*d.* is the present value of the lessor's interest in the house.

Example 2.—A lease of an estate is granted on three of the best lives that can be found with a perpetual right of renewal, subject to an annual conventionary rent of 2*l.* 10*s.* an heriot, on the death of each life 5*l.* and the fine for renewal 50*l.*; required the present value of the landlord's interest in the estate, the rate of interest 6 per cent.

Answer.—The value of the best life, from observations made at Carlisle, is 14·526, the value of the perpetuity $\frac{100}{6} = 16{\cdot}667$, and 16·667 — 14·526 = 2·141; then by the rule, we have 14·526 : 2·141 :: 3 (5 + 50) : 24·32, the present value of all the heriots and fines; the conventionary rent multiplied by the perpetuity is $2\frac{1}{2} \times \frac{100}{6} = \frac{125}{3} = 41\cdot666$, &c.

Therefore, 24·32 + 41·666, &c. = 65·986 = 65*l.* 19*s.* 8¾*d.* is the present value of the landlord's interest in the estate.

Example 3.—A lease of an estate is granted on three of the best lives that can be found, with a perpetual right of renewal with the best lives also, subject to a fine of 200*l.* for each renewal; required the present value of the landlord's interest in the estate, according to observations made at each of the places mentioned in the foregoing table, interest being either 4 or 5 per cent.

Answer.—The numbers standing against Carlisle, and under 4 and 5 per cent. are ·78936 and ·57355, each of which being multiplied by the fine for renewal (200*l.*) will produce 157·872*l.* and 114·71*l.* respectively, which are the present values of the landlord's interest in the estate, according to the Carlisle tables. In like manner, the remaining values are found, and the whole set down as under.

According to the	4 per cent.				5 per cent.		
Carlisle tables	157*l.*	17*s.*	5¼*d.*	....	114*l.*	14*s.*	2¼*d.*
French tables	188	11	2¼	....	140	2	11
Swedish tables	201	1	4¼	....	148	11	11
Northampton tables	249	5	7	....	188	2	6¾
Demoivre's hypothesis	288	10	4¾	....	221	10	5¾
London tables	314	12	8	....	239	3	2¼

Although the last two mentioned tables are not much used at present, yet seeing the different results produced by the preceding four, it would, in my opinion, be proper to advise such persons who may have occasion to make bargains wherein the probabilities of lives are concerned, to stipulate that a particular table should be the ground work of their agreement; then, whatever might be the consequence, it must be abided by.

Would not a standard set of tables of mortality be as desirable and beneficial in this country, as standard tables of weights and measures?

An approximate value of an annuity on a single life may be found as follows within particular limits:

Take half the complement of the age to 86, and find the present value of 1*l.* per annum, which corresponds to the half complement, and you will have the value of the given life for an annuity of 1*l.* nearly, the rate of interest being 4 per cent. If the rate of interest be 5 per cent. take half the complement of the given age to 85, and proceed as above, according to the Carlisle tables.

If the tables of M. de Parceiux be used as given in Mr. Baily's Annuities, take half the complement to 85 for 4 per cent. and to 84 for 5 per cent. The following table will show how near the true and approximate values agree :

Carlisle Table 4 per cent.					Parceiux' Table 4 per cent.				
Age.	Year's purchase by table.	Half comp. to 86.	Year's purchase by rule.	Difference.	Age.	Year's purchase by table.	Half comp. to 85.	Year's purchase by rule.	Difference.
7	19·792	39½	19·688	—·104	10	19·008	37½	19·255	+·247
10	19·585	38	19·368	—·217	15	18·502	35	18·665	+·163
15	18·956	35½	18·786	—·170	20	17·938	32½	18·011	+·073
20	18·363	33	18·148	—·215	25	17·420	30	17·292	—·128
25	17·645	30½	17·440	—·205	30	16·810	27½	16·496	—·314
30	16·852	28	16·663	—·189	35	16·084	25	15·622	—·462
35	16·041	25½	15·802	—·239	40	15·133	22½	14·654	—·479
40	15·074	23	14·857	—·217	45	13·904	20	13·590	—·314
45	14·104	20½	13·809	—·295	50	12·526	17½	12·412	—·114
50	12·869	18	12·659	—·210	55	11·173	15	11·118	—·055
55	11·300	15½	11·385	+·085	60	9·713	12½	9·685	—·028
60	9·663	13	9·986	+·323	65	8·039	10	8·111	+·072
65	8·307	10½	8·495	+·188	70	6·394	7½	6·367	—·027
70	6·709	8	6·733	+·024	75	4·945	5	4·452	—·493
75	5·239	5½	4·847	—·392					

Carlisle Table 5 per cent.					Parceiux' Table 5 per cent.				
Age.	Year's purchase by table.	Half comp. to 85.	Year's purchase by rule.	Difference.	Age.	Year's purchase by table.	Half comp. to 84.	Year's purchase by rule.	Difference.
7	16·790	39	17·017	+·227	10	16·213	37	16·711	+·498
10	16·669	37½	16·789	+·120	15	15·865	34½	16·283	+·418
15	16·227	35	16·374	+·147	20	15·469	32	15·803	+·334
20	15·817	32½	15·903	+·086	25	15·117	29½	15·256	+·139
25	15·303	30	15·372	+·069	30	14·693	27	14·643	—·050
30	14·723	27½	14·770	+·047	35	14·175	24½	13·946	—·229
35	14·127	25	14·094	—·033	40	13·459	22	13·163	—·296
40	13·390	22½	13·326	—·064	45	12·487	19½	12·273	—·214
45	12·648	20	12·462	—·186	50	11·363	17	11·274	—·089
50	11·660	17½	11·482	—·178	55	10·242	14½	10·139	—·103
55	10·347	15	10·380	+·033	60	9·003	12	8·863	—·140
60	8·940	12½	9·128	+·188	65	7·535	9½	7·415	—·120
65	7·765	10	7·722	—·043	70	6·055	7	5·786	—·269
70	6·336	7½	6·124	—·212					

It will be observed that the half complements form arithmetical progressions whose common difference is half of unity. The student must be careful not to mistake the *half complement* for the *expectation* of the corresponding lives.

ARTICLE V.

Tables of Temperature, and a Mathematical Development of the Causes and Laws of the Phænomena which have been adduced in Support of the Hypotheses of "Calorific Capacity, Latent Heat," &c. By John Herapath, Esq.

(*Concluded from* vol. ii. p. 462.)

Theory of Evaporation.

PROP. XXIII. PROB. VIII.

The weights of two quantities of water and steam in contact being given, and their common temperature, it is required to determine the temperature and quantities of water and steam which will result from the mixture with them, in a given space, of a given weight of any other body whose baromerin and temperature are known; no chemical action being supposed to take place.

In the solution of this problem, we suppose no foreign cause to influence the results either by "radiation" or otherwise.

Case 1.—Let us conceive the matter of the vessel to have no effect. Put W, w, and t, for the primitive weights of water and vapour and their common temperature; and let Q, q, t, and Q′, denote the weight, baromerin, temperature, and volume or magnitude of the other body. Let also W′ denote the weight of water after the mixture, and τ its temperature. Then by the principles we have already demonstrated

$$\tau\,(Q\,q + 11\,W + 11\,w - 5\,W') = Q\,q\,t' + (6\,W + 11\,w)\,t \ldots\ldots A$$

Again: suppose v be the volume of unity weight of water at the temperature T, and that it becomes $v + \psi\,(\tau - T)$ at any other temperature τ; then we have $W'\,v + W'\psi\,(\tau - T)$ for the volume occupied by the water in the mixture. In the same manner, if $r\,v$ represent the volume of unity weight of vapour at the temperature T and elasticity E, we shall have $\frac{E\,\tau^2}{T^2\,\phi\,\tau}\,r\,v\,(W + w - W')$ for the volume of vapour in the mixture by Prop. 15 of the present paper, and the Theorem in the *Annals* for July, 1816. Whence putting S for the given space in which the mixture is made, we have, by thequestion, supposing Q′ at the temperature τ becomes $Q' + \chi\,(\tau - t')$

$$\chi\,(\tau - t') + Q' + W'\,\{v + \psi\,(\tau - T)\} + \frac{E\,\tau^2}{T^2\,\phi\,\tau}\,r\,v\,(W + w - W') = S \ldots\ldots B.$$

Eliminating from A and B the quantity W′, we shall have an equation involving only τ and known quantities. The value of τ being determined from this equation will afterwards enable us to determine that of W′, and consequently the weight of the vapour.

Case 2.—When the matter and temperature of the vessel are to be taken into account, we must substitute for them in the equation A in the same manner as we have for the matter and temperature of the body Q. And in the equation B, we must make an allowance in the capacity S for the effect of contraction or expansion on the capacity in changing from the temperature of the vessel to the temperature τ. By this means we get two new equations A_1 and B_1 from which eliminating W′, as before, we obtain the value of τ, and thence the other requisites of the problem.

Cor.—By the same process we can find the effect of the mixture of a quantity of water and vapour at one temperature with another given quantity of water and vapour at another temperature, the capacity, temperature, and matter of the vessel being given. From this we may find how much water at a given temperature it would take, when intromitted into a vessel filled with water and vapour of a given temperature, to reduce the temperature or tension of the mass from one given quantity to another, as well as many other things of a similar kind. These are, however, problems which, though very interesting, we cannot, in the present state of science, exhibit in finite equations; and for this reason I think it unnecessary here to pursue the inquiry.

Scholium.

The theorems contained in this part of the present paper, contain all that is necessary to be known relative to the force, condensation, and laws of aqueous vapour in the theory of the steam engine. A moderate share of ingenuity will enable engineers to apply the principles I have developed to the resolution of almost, or, perhaps, every problem that can arise or be proposed relative to the power and operations of that useful instrument. I had at one time thoughts myself of entering pretty fully into the resolution of the more practical cases; but perceiving this was a matter of no unusual difficulty, when the physical part was clearly expounded, I have thought it better to employ the little time I have had to devote to these things to a more luminous development of my physical principles, and of the evidence I have had at hand to support them, than to cramp one part of the subject without advantage to the other.

Sometime ago I had promised myself to close the present paper with the last volume of the *Annals;* and, for this purpose, had availed myself of the kind indulgence of the proprietors and editor to occupy in the two or three last numbers larger portions of that work than are usually allowed to single individuals.

However, as I approached the end of the part which has just been published, finding it utterly impracticable to close the paper within the prescribed limits, I was in part prevailed on by the Rev. Mr. Trimmer to undertake to try the success of my inquiries on the laws of Chemical Combination, Decomposition, &c. In this proposed inquiry, my attention was intended to be particularly directed to the investigation of the merits of the atomic theory, which has been of late years so ably advocated by those distinguished philosophers, Richter, Dalton, Gay-Lussac, Thomson, &c. Unhappily other circumstances have so intervened to delay my taking up the inquiry that I find it impossible to prepare for the present number in the manner I could wish; and, therefore, I have thought it preferable to make the development of my views the subject of a future communication. However, it may not be uninteresting to philosophers to know, that though I have had but the opportunity of a few scattered hours to consider the subjects, my principles have enabled me to succeed in the demonstrations of the leading laws and phænomena of Chemical Union. For instance, I have reason to believe I have perfectly succeeded in demonstrating Dr. Richter's phænomena of saturation, which are the foundation of Dr. Wollaston's sliding rule of chemical equivalents; Dr. Henry's laws of the absorption of gases; Mr. Dalton's theory of definite proportions; and a variety of other things which flow from them. But of all the phænomena I have hitherto demonstrated in this part of my inquiries, none have pleased me so well as the proof I have been able to give of M. Gay-Lussac's observation of the laws of volume in the chemical union or disunion of gaseous bodies. This proof, besides confirming the general views of Gay-Lussac, illustrates my theory of gravitation by a train of facts not less beautiful than unexpected; nor when contemplated as the simple consequences of a simpler cause, less splendid than simple, nor less simple than consistent and evident. Independently of corroborating the consequences deduced from observation, the full development appears likely to lead us to an uninterrupted unclouded view of changes and phænomena more refined than have yet been conceived; but yet marked with that simplicity which so strongly characterizes the ever conformable operations of nature.

One singular thing flows from my investigations on this subject perfectly consistent with what I had anticipated in other phænomena, and have mentioned in page 257, and other places, in the last volume of the *Annals*: namely, that almost all chemical combinations are preceded by a disunion of the particles of the component bodies. Not only does it appear that the particles of the heavier gases, as oxygen and chlorine, which have commonly been conceived to be simple bodies, are decomposable, but also that the particles of hydrogen are likewise decomposable; and that in the formation of water they are actually trisected. In the combination of hydrogen with chlorine to form muriatic acid gas, the particles of hydrogen are still further divided; and I have reason to believe there are other cases in which the division is carried to a much greater length. I have often sought for some satisfactory reason that philosophers might have for ranking hydrogen among the elements, but have not yet met with any. Sir H. Davy thinks the great levity of hydrogen is an argument in favour of its elementary nature. Levity may certainly induce us to think that the body in which it most predominates approximates the nearest to an elementary substance; but in such a case as this, it can, it appears to me, by no means be considered a proof, or even a probable argument, of hydrogen being an element. For instance, if hydrogen be esteemed an element because it is about $14\frac{2}{5}$ lighter than common air, among what bodies must we rank light whose levity to that of hydrogen has, in the strictest sense of the word, no appreciable ratio? Yet this very body, light, we have good reasons for believing, consists of molecules of at least seven different sizes; and it is not absolutely certain that even these molecules are indivisible atoms. Even the implied size of the particles of hydrogen has, I think, from its levity, been considerably overrated. According to the theory I have demonstrated, it appears that the diameter of a particle of hydrogen is more than two-thirds that of a particle of nitrogen. Surely then there can be no argument gained in favour of the simplicity of that body from the smallness of its particles, especially as nitrogen is considered to be a compound.

There is no direct method of ascertaining from the old theory the relative sizes of the different gaseous particles; but from the results of some experiments it seems possible to arrive at something like a ratio. Thus in the formation of muriatic acid gas from the exposure of equal parts by volume of chlorine and hydrogen to common day light, we have evidence enough, if the old theory of gaseous repulsion be true, to demonstrate that equal volumes of hydrogen and chlorine contain equal numbers of particles; and, consequently, that the diameters of the hydrogen and chlorine particles are respectively as 1 and 3·3. These dimensions, therefore, could be esteemed very little more an argument in favour of the elementary nature of hydrogen than those drawn from my theory. In fact I can discover no one phænomenon whatever which sanctions the probability of hydrogen being a simple body. Its combustibility is by no means an argument in favour of such an idea; and the size of its particles, any how computed, is rather an argument of the contrary. The superior disposition it exhibits to combine with other bodies, which Sir H. conceives is partly demonstrative of its elementary nature, I think I shall be able to prove is due to a very different cause.

Another important consequence seems to flow from my inquiries into the atomic theory; namely, that hydrogen, oxygen, aqueous vapour, and in fact all pure gases, are very nearly, if not accurately, homogeneous. Hence the theorem which I have given, p. 454 of the last volume, for finding the baromerin of homogeneous airs, is, as I suspected, very nearly, or, perhaps, perfectly correct in all pure gases and vapours, whether they be simple or compound.

This theorem for practice will be more convenient in this form $\frac{11}{2G}\sqrt{G}$; in which G represents the specific gravity of the gas, that of hydrogen being unity, and the baromerin of water also unity.

In an early part of the present paper, I had hinted my intention to undertake the refutations of the modern doctrines of "capacity and latent heat." This I have still in contemplation to do; but as the present communication has already been extended to a very unusual length, it has appeared advisable to defer the execution of this intention to a future period, and to close the present paper with a brief recapitulation of the things I have in the course of the two papers developed and demonstrated, with the authority of the phænomena annexed. By this recapitulation, I shall give a kind of index to the papers, and at the same time afford philosophers an opportunity of seeing by a glance of the eye the great variety of phænomena I have succeeded in developing; by which means they will be the better able to judge of the merits of the theory I have embraced. I shall say nothing of the theory of collision, because it relates to perfectly hard bodies, and does not, therefore, properly speaking, come under the class of ordinary phænomena.

ANNALS, VOL. I. (*New Series.*)

PHÆNOMENA

Developed.	*Confirmed by*
1. P. 343.—The elasticity of a given portion of gas is the same whatever be the figure of the vessel in which it is contained, provided the capacity and temperature be the same.	This is generally admitted, but I do not know that it has ever been made the subject of direct experiment.
2. P. 344.—Other things being alike, the elasticity of any gas is directly as the compression, or reciprocally as the space.	By Boyle in air, and in other gases by others.
3. P. 345.—The elasticity is as the square of the temperature directly and simple of the space inversely.	The law of Boyle united to the experiments of De Luc and myself.
4. P. 346.—Elasticities being equal, the spaces are as the squares of the temperatures.	De Luc and myself.
5. Ibid.—Equal changes of temperature equally affect equal volumes of all gases, other things being alike.	Dalton and Gay-Lussac.

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Phænomena	
Developed.	*Confirmed by*
6. P. 346.—If the elasticities of any two gases have an invariable ratio, and their temperatures an invariable ratio, their volumes will have an invariable ratio.	No one directly, but the experiments of De Luc and myself in consequence.
7. Ibid.—The temperature of water freezing is to that of water boiling as $\sqrt{8}$ to $\sqrt{11}$.	De Luc and myself.
8. P. 347.—The same results are obtained by measuring the temperatures by the elasticities, under an invariable volume, as by the volumes under an invariable compression.	Mariotte, Dulong and Petit.
9. P. 349 and 350.—Prop. 10, and its corollaries.	Have not been proved directly by any one, but may be inferred from the general law of temperature confirmed by the experiments of De Luc and myself.
10. P. 401.—Sudden condensation in gases produces heat, sudden rarefaction cold; if the condensation or rarefaction be slowly made, no perceptible change takes place.	Mollet and Dalton.
11. P. 402.—Gases transmit heat rapidly, but feebly in right lines.	Leslie.
12. Ibid.—The lighter the gas, the more rapidly it abstracts temperature under certain circumstances.	Leslie, Davy, Dulong and Petit.
13. P. 403.—The baromerin of hydrogen is four times greater than that of oxygen.	Crawford's ratio of capacities nearly confirm it.—(See vol. ii. p. 211.)
14. Ibid.—Two particles of oxygen go to one of hydrogen to form water.	Ditto, ditto.
15. Ibid.—General theorem of temperature.	De Luc's and my experiments.—(See p. 405, and vol. ii. p. 100.)
16. Ibid.—Absolute cold 448° Fahr. below 32° Fahr.	Ditto, ditto.
17. P. 406.—Megethmerin of mercury to water as 1 to 2; masses of particles as 27 to 1.	Calculations and comparisons with experiments of Henry (Dalton), p. 406; also vol. ii. p. 208.
18. Ibid.—Phænomena of "capacity for caloric" due to megethmerin.	Ditto; also vol. ii. p. 202, 203, 445, 448, 453, 454, and 460.
19. P. 407.—Phænomena of "latent heat" due to aggregation and decomposition of particles.	Expounded briefly, p. 407, fully in theory, vol. ii. p. 256, and verified in theory, p. 443, &c. by calculations from experiments of Black, Watt, Rumford, Kirwan, Irvine, Lavoisier and Laplace, Thomson, Ure, Crawford, Southern, &c.
20. P. 408.—Law of attraction in very small bodies at sensible distances inversely as the square of the central distance, directly as the mass of the attracted body, and the temperature being the same, as the mass of the central body.	Newton.
21. Ibid.—Particle attracted by a sphere at a distance as mass of particle and mass of sphere directly (temperature being invariable) and square of central distance inversely.	Newton.
22. Ibid.—Law of attraction on a perfectly solid imperviable cylinder shown.	Cannot be confirmed but by induction.

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PHÆNOMENA

Developed.	*Confirmed by*
23. P. 409.—Two homogeneous spheres of the same temperature, attract one another as their quantities of matter directly and square of the distance of their centres inversely.	Newton.
24. P. 410.—Present theory of gravitation would not produce the least sensible effect on the system in a period many million times 2857796067672610 years.	Newton and Laplace that the system has apparently the utmost stability, and without foreign interference will continue the same for many thousand years.
25. Ibid.—Activity of present theory of attraction so great, that it would act equally intense on bodies moving either with or against it, with a velocity at least many million million times faster than light.	Laplace has proved that the activity of gravitation must be at least six million times greater than that of light.
26. Ibid.—Resistance of the gravific fluid can produce no sensible effect on the system in a period of many million years.	Newton and Laplace show, that if there be any resistance, it is too small to become sensible in several thousand years.
27. P. 411.—Attraction is greater the greater the temperature of the attracting body.	Euler, in the Refraction of Light; Laplace's Computation of the Ann. Equa. of the Moon;* Diminution of Planetary Attraction in receding from the Sun; and in the small Action of Comets.
28. P. 412.—Ellipticity of the earth by old theory of uniform attraction should be too little by the pendulum, and too great by Newton's calculation.	By pendulum $\frac{1}{336}$; by most of the admeasurements $\frac{1}{310}$; and by Newton's calculation $\frac{1}{230}$.
29. P. 414.—Attraction between particles when they nearly touch increases much faster than the squares of the distances diminish.	Many chemical phænomena; also by Newton, Desaguliers, Laplace, &c.
30. Ibid.—Affinity and phænomena of chemical action arise from figures of the component particles.	Idea always entertained by our best philosophers, but hitherto has not been proved. My late inquiries into the laws of combination between gases with gases, and gases with fluids, &c. will, I think, demonstrate it. A tolerably fair proof may be drawn from my theory of evaporation, vol. ii. p. 363, &c.

ANNALS, VOL. II. (*New Series.*)

PHÆNOMENA

Developed.	*Confirmed by*
31. P. 99 and 100.—Results of two of De Luc's, and one of my own experiments. Mean difference of the three from my theory $\frac{1}{34}$th of a degree Fahr. Mean ditto from old theory, 5·4° Fahr. Ratio of these differences as 1 to 162.	Also by other experiments made by myself on mercury.
32. P. 98 to 103, and 201 to 211.—	The experiments of Dalton and myself.

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* There has been lately a prize obtained by two French mathematicians for a set of lunar tables completely theoretical; I do not know what they make the Ann. Equa.; I have not seen any account of their computations.

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Phænomena

Developed.	*Confirmed by*
Mathematical theory of phænomena connected with "capacity for caloric," in Prop. 1, 2, 3, 4, 5, and 6, and their corollaries.	
33. P. 209.—Usually bodies that expand by heat have greatest baromerin or "capacity."	Quotation from Davy's Elements of Chemical Philosophy.
34. P. 210.—Baromerin or capacity of gases and airs usually greater than that of solids or fluids; a general but not universal rule.	Crawford and others.
35. P. 258.—During the liquefaction of such solids as ice the temperature is stationary.	Do not know who first observed it, but I think it was Fahrenheit.
36. Ibid.—If a given weight of a fluid at a given temperature will just liquefy a given weight of a solid in one mass, it will just and no more than do it if the solid be in any number of pieces, or pulverized.	I am not acquainted with the original discoverer of this fact. I think it was Black.
37. P. 259.—The temperature of ebullition in fluids is constant.	Hooke.
38. Ibid.—The liquefaction of solids and the vaporization of fluids are usually attended with an apparent diminution of temperature; and *vice versâ* the solidification of fluids and the condensation of vapours with an apparent increase of temperature	Black.
39. P. 260.—During the time of actual solidification, the temperature is constant.	Black, I believe.
40. Ibid.—Water may be cooled down below its freezing point without solidifying. If shaken, it solidifies in part, and temperature ascends to 32° Fahr.	Mairan, Fahrenheit, Gay-Lussac, Black, Blagden, and Thomson. By the experiments of the last, the theory is confirmed numerically, vol. ii. p. 449.
41. P. 261.—Water cooled below 32° Fahr. may be stirred without freezing.	Blagden.
42. Ibid.—Water with opaque bodies floating in it freezes, if cooled only a few degrees below 32° Fahr.	Blagden.
43. Ibid.—Water gently cooled below 32° Fahr. will not freeze, suddenly cooled will.	Blagden.
44. Ibid.—Piece of ice thrown into water cooled below 32° Fahr. causes it to freeze.	Blagden. N.B. Neither when writing this part nor since have I had an opportunity of examining the circumstances of this phænomenon attentively.

When writing this part, a very important phenomenon, the expansion of water as it cools below 40° Fahr. escaped my notice. M. Biot, however, in the Traité de Physique, tom. i. p. 254, has so nearly approached to my ideas on the subject, that were I to describe it I should do very little more than transcribe the explanation he has given.

45. P. 262.—Rise of temperature in condensation of airs and solidification of fluids; and diminution of temperature in liquefaction of solids and vaporization of fluids are *generally* but not *necessarily* true.	Phænomena expounded in the following pages.

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Phenomena

Developed.	*Confirmed by*
46. P. 262.—Fluidity results either from sphericity of particles, or the extent of their aberrations overcoming the influence of their irregularity of figure. Of course, solidity results from irregularity of figure and smallness of extent of corpuscular vibrations.	
47. Ibid.—Carbonic oxide and oxygen unite and form a gas (carbonic acid) with a baromerin less than that of either of the component gases, and with a greater specific gravity.	Berard and De Laroche.
48. P. 264.—Solids may, under peculiar circumstances, be converted into airs with an increase of temperature.	Explosion of gunpowder, &c.
49. P. 265.—All changes which produce a greater number of particles out of the same quantity of matter occasion a diminution of temperature; all changes which diminish the number of particles increase the temperature.	
50. Ibid.—Chemical rule "that all chemical changes produce an alteration of temperature," is pretty general, but not universal; and not a law of nature.	Discussion of parallel phenomena.
51. P. 267.—Airs have generally their particles less than those of fluids. Hence in a given weight, there is generally a greater number of particles in an air than in a solid or fluid.	Phenomena of "specific heats" which shew that the baromerins of airs usually exceed those of fluids and solids.
52. P. 267, 268.—Condensation of vapours is owing to the irregularity of the figures of their particles, and, therefore, occasioned by a diminution of temperature.	Well known phenomenon that lowness of temperature produces condensation.
53. P. 268.—Difference between vapours and gases, is merely in the figures of their particles.	The best proof of this is the coincidence of the consequences in my theory of evaporation with the experiments of Dalton.
54. Ibid.—Vapours unconnected with their fluids, and at all higher temperatures than that of their condensation, are perfect gases, and follow the same laws.	Dalton.
55. Ibid.—Mixture of different vapours, or of vapours and gases, if no chemical action take place, has the same law as mixture of gases.	Dalton and Gay-Lussac.
56. P. 270.—Calculations of specific gravity of steam at different temperatures.	Experiments of Southern and Sharpe.
57. Ibid.—Pressure aids condensation of vapours; this effect of pressure diminishes as the temperature increases.	Dalton.
58. P. 271.—Two gases separately incondensible mixed together may easily condense.	Mixture of sulphurous acid gas and hydrogen.
59. P. 272.—Temperature of ebullition of all fluids is increased with an increase, and diminished with a diminution of pressure.	Robinson.

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PHENOMENA

Developed.	*Confirmed by*
60. P. 272.—The Fahrenheit temperature of ebullition increases and decreases more rapidly than the compression.	De Luc, Betancourt, Shuckburgh, &c.
61. P. 273.—The temp. of the liquefaction of solids is not influenced by external pressure.	Is a well-known fact, but I do not know the discoverer, or who has made direct experiments on it.
62. Ibid.—Ebullition arises from violent decompositions in the interior, not at the surface of the fluid.	Hamilton.
63. P. 363.—Evaporation is a decomposition of the superficial particles arising from the mutual collisions of the particles, or the temperature of the body.	It is generally, I think, admitted, that evaporation takes place at the surface.
64. P. 365.—In equal or unequal, but great depths, the evaporation is at the same temperature proportional to the exposed superficies.	Dalton, Leslie, &c.
65. Ibid.—Two portions of the same fluid cooled from any common to any other common temperature by evaporation alone, lose quantities proportional to the original quantities of the fluids; conversely two portions of the same fluid losing by evaporation quantities proportional to their weights, would be equally reduced in temperature, if their temperatures were at first equal.	Shown in the scholium to be consistent with phænomena.
66. P. 368.—The incremental condensation of any vapour at the same temperature in vacuo is, *cæteris paribus*, as its elasticity.	The proof of these laws appears in the perfect agreement of the whole theory with the experiments of Dalton, Gay-Lussac, De Luc, &c.
67. P. 369.—The megethmerin being the same, the increment of condensation is as the cube of the temperature.	
68. P. 370.—The incremental condensation is as the elasticity and temperature conjointly.	
69. Ibid.—The incremental condensation in the same vapour is as the specific gravity and cube of the temperature conjointly.	
70. Ibid.—Other things being alike, the mixture of any quantity of gas with vapour in a given space produces no effect on the celerity of condensation, however much it may augment the elasticity.	
71. P. 372.—If there be sufficient fluid, and the temperature the same, the tension of the vapour will be the same whatever space it occupies.	Dalton.
72. Ibid.—Pressure has no effect in augmenting or diminishing the absolute evaporation of any fluid, the temperature being the same.	The truth of this can appear only as a part of the general theory.
73. P. 373.—Calculations from the preceding theory agree with six experiments at a mean to within about fifteen parts in a thousand of a grain.	Dalton.

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Phænomena

Developed.	*Confirmed by*
74. P. 374.—A theorem expressing the apparent evaporation. N. B. This theorem is misprinted; it should be $\frac{e}{t\,\tau'}$; $(T\,\tau - T'\,E)$.	
75. Ibid.—Vapours in vacuo can only support a given pressure according to the temperature, but mixed with sufficient gas can support an indefinite one.	Dalton.

Let $\tau = \varphi\,t$ be the tension of the vapour at the temperature t, and E the elasticity of any gas occupying the space S at the temperature T. Then if e be the elasticity of the mixture, and s the space occupied, we have $e = E\,\frac{S\,t^2}{s\,T^2} + \varphi\,t$. This is a more general equation than that given by M. Biot in the Traité de Physique. It is, however, like his and all others of this kind, not mathematically true, in consequence of not taking into account the quantity of gas absorbed at the temperature t. I shall probably, in a future communication, consider this circumstance, and show how to make the necessary allowance.

Developed.	*Confirmed by*
76. P. 275.—Rarefaction of air promotes desiccation.	Leslie, &c.
77. Ibid.—Method of obtaining a Torrecellian vacuum.	Has been partially tried by Smeaton, &c.
78. P. 376, 377, and 378.—Formulæ for determining the apparent and absolute quantity of vapour at any time in the atmosphere, with a method pointed out of verifying by them the truth of the theory.	I shall be glad to see these formulæ brought to the test of experiment.
79. P. 381.—Formulæ of the effect of cold water in drying a room.	
80. P. 382.—Apparent evaporation is proportional to the velocity of the current of air passing over the surface of the fluid.	Leslie.
81. P. 384.—Either a current or an agitated air increases apparent evaporation, and diminishes the temperature.	Leslie and Dalton.
82. Ibid.—Water of a low temperature, or even ice in a current or an agitated air, may lose more weight by evaporation in a given time than water of a higher temperature in a still atmosphere.	I think the superior evaporation of ice is to be found in Clare, Rowning, or Hamilton.
83. P. 384, 385, 386, 387, and 388.—Water commonly colder than the atmosphere.	Wells and myself.
84. P. 435.—Theorem for the tension of aqueous vapour in contact with its fluid at all temperatures.	Experiments of Robison, Dalton, Ure, and Southern, but principally Ure's.
85. P. 440.—Temperature of no evaporation — 130° Fahr.	The theorem confirmed by Ure's experiments.
86. P. 441.—The temperature of ebullition higher than the temperature of tension.	Shown from the experiments of Robison, Dalton, and De Luc.
87. P. 444.—Baromerin of ice to baromerin of water as 19 to 22.	Experiments of Black, Kirwan, Irvin, Thomson, and Lavoisier and Laplace.
88. P. 445.—"Capacity" of water being 1, that of ice is ·86.	Mean of Irvin and Kirwan, ·85.
89. P. 447.—Theory of the calorimeter. Calculation of capacity of iron plate 116.	Lavoisier and Laplace, ·111.

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PHÆNOMENA

Developed.	*Confirmed by*
90. P. 449.—Theoretical calculations of water frozen by agitation in two experiments of Thomson.	Differ in one instance $\frac{1}{132}$d part, and in the other a $\frac{1}{420}$th.
91. P. 451.—Baromerin of vapour to that of water as 11 to 6.	Mean diff. from four experiments by Thomson, Ure, and Rumford, $\frac{7}{40}$th of a degree Fahr.
92. P. 453.—Theoretical determination of the "capacity" of aqueous vapour 1·83, that of water being 1.	Crawford's experiments give 1·55.
93. P. 454.—Formula for determination of baromerins of homogeneous gases compared to that of water, and nearly of all gases.	Has been verified in aqueous vapour by Crawford's experiments.
94. Ibid.—Specific heats of the lighter airs exceed those of the heavier.	Crawford, De Laroche, and Berard.
95. P. 456.—Theoretical calculation of vapour lost by suddenly opening water heated to 400° Fahr.	Agrees with Watt's experiment to a $\frac{1}{17\frac{1}{2}}$th part of the whole water.
96. P. 460.—Mean capacity of water between 32° and 122° Fahr. to ditto between 122° and 212°, as 15·1 to 14.	De Luc's and my experiments as 15 to 14.
97. Ibid.—Capacities of water and mercury decrease with ascent of temperature, and *vice versâ*.	De Luc, Dalton, Ure, and myself.
98. Ibid.—The greater the ratio of water to vapour in the experiment, the less, *cæteris paribus*, will be the numerical value of latent heat.	Experiments of Ure and Rumford.
99. P. 461.—The higher also either or both the temperatures, the greater, under equal circumstances, will be the value of latent heat.	Have no experiments to confirm or disprove it.
100. Ibid.—Combining the notion of caloric with our formula, the temperature has less influence. A calculation gives 11° under certain circumstances.	Southern's experiments under similar circumstances give an increase of 8°. The nature of the thing is such that this may be looked on as a coincidence.

These are the leading facts I have deemed it needful to select. Several others I have, for the sake of brevity, particularly in the latter part, it will be seen, omitted. I intend to make no comment on the number, extent, and variety, of these testimonies, or of the subjects to which they relate, but shall leave philosophers to form their own opinion of the merits of a theory which can in so great a number of instances, and on such subjects, be the faithful representative of phænomena.

Cranford, Dec. 12, 1821. J. HERAPATH.

P. S. Since finishing the preceding paper, I have computed from Dr. Ure's theorem, which it seems accurately coincides with experiment at 210° and 220° Fahr. the tension of steam at 212°, and I find it 30·1413, instead of 30, the compression due to ebullition at 212°. This, therefore, confirms what I have said, p. 441 and 442, respecting the temperatures of tension and ebullition, and respecting Dr. Wollaston's thermometer. The neglect of the distinction I have alluded to will commonly make the Doctor's instrument err nearly 75 feet, or 25 yards, in the heights determined.

Dr. Ure's theorem will, in general, be much more commodious for practice in the following forms than in the one he has given; namely, $\tau = 28·9 \times (1·34 - ·0005\,F)^{\frac{F-210}{10}}$ or $\log. \tau = \log. 28·9 + \frac{F-210}{10} . \log. (1·34 - ·0005\,F)$ where τ is the tension, and F the Fahr. temperature in degrees.

ERRATA

Discovered in those Papers of Mr. Herapath, published in the Annals of Philosophy, which have his Name to them.

Page.
1821. April.—274, line 2, from top, *for* FRS. *read* VPRS.
line 13, *for* shown all, *read* shown that all.
276, line 18, *for* an. equa. *read* Ann. Equa.
line 11, from bottom, *for* receive, *read* conceive.
278, line 15, *for* revolved, *read* turned.
282, Cor. to Def. 1, *for* primary particles, *read* primary parts.
285, line 10, from top, *for* menumentum, *read* momentum.
287, line 11, in Demons. of Prop. 3, *for* $(a-b)$, A, *read* $(a-b)$ A.
line 13, *for* body. This, *read* body, this.
June.—409, line 18, from bottom, *for* même qui, *read* même que.
line 16, *for* des fois, *read* de fois.
July.— 55, column 5, *dele* ditto at Paris.
for Pekin, *read* Paris; and immediately beneath, in the blank, *supply* ditto at Pekin.
Aug.— 89, column 5, *for* melts, *read* melt.
opposite 1260 true temp. *supply* Oil of turpentine boils.
91, column 5, *dele* oil of turpentine boils.
96, line 17, from top, *for* Mr. *read* Dr.
101, line 8, *for* or thought, *read* or had thought.
Sept.—205, lines 11 and 12, *dele* the commas after given and mixed.
208, line 5, from bottom, *for* presents, *read* present.
Oct.—265, line 11, from top, *for* showt hat, *read* show that.
307, in the note, *for* ausam, *read* ansam.
Nov.—364, line 4, *for* By his, *read* By this.
365, last word of Demons. of Prop. VII. *for* arcs, *read* areas.
374, line 20, *for* $\frac{s}{t\,\tau'}(\mathrm{T}\,\tau - t\,\mathrm{E})$, *read* $\frac{s}{t\,\tau'}(\mathrm{T}\,\tau - \mathrm{T}'\,\mathrm{E})$.
383, line 2, from top, *for* world, *read* wind.
Dec.—435, line 7, *for* 16. *read* ·16.

$$\text{In Ex. 2, } for \ \frac{3\cdot0531169}{7\cdot4445621}, \ read \ \begin{array}{c}3\cdot0531169\\ 7\cdot4445621\\ \hline\end{array}$$

line 1 of Ex. 3, *for* as, *read* As.
438, line 20, from top, *for* 2·934°, read 293·4°.
446, line 9, from bottom, *for* no solidification, *read* no total solidification.
450, in NB, *for* 888° — See, *read* 888°, see.
457, line 2, Case 1, *for* Suppose F_1, *read* Suppose F.
465, line 11, from bottom, *for* side? *read* reside?

ARTICLE VI.

Reply to X. By John Herapath, Esq.

(To the Editor of the *Annals of Philosophy.*)

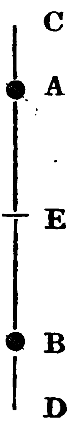
DEAR SIR, *Cranford, Dec.* 14, 1821.

X. informs us that he wishes the objections he has advanced against my theory, or rather the difficulties he has met with in the perusal of it, "to be received without offence." I assure him that any observations on my works published in the same friendly spirit in which his appear to be, whether I reply to them or not, I shall always respect; and however much they may differ from my ideas, shall uniformly regard them as the candid effusions of a liberal mind. It is, therefore, my request, that X. receive this reply to his "Remarks" with the same feeling of friendly good will with which I can assure him it is dictated; and should my observations in any place appear harsh, which I trust they will not, I hope this preface will be admitted a sufficient apology for what is not the effect of intention.

X. acknowledges in his second paper, in the *Annals* for Nov. that he has "found" in his first, "some observations" (misrepresentations) "which he would wish to retract;" but adds, that they are "only one or two of little or no importance." In this, I must beg to differ from him both as to number and importance. However, as he has made so candid an acknowledgment, I will not press the matter.

In his first paper, p. 224, *Annals* for Sept. he suspects me of having argued falsely from my principles; and in his second, repeats the charge. He says: "We may certainly grant that the elasticity varies as the action of the particles against a given portion of the surface containing the gas, but *it may fairly be questioned*, whether this action can be measured by the momentum × the number of returns." Does X. perceive that this is not demonstrating, but merely surmising, that I am wrong? The only reply I should, perhaps, make to such an observation is, "Lay aside surmise; endeavour to show I am wrong; and I will try to prove I am right." However, if X. will consult p. 341 and 342, *Annals* for May, I think he will find I am not mistaken in the conclusion I have drawn. My object is to compare the effect of a gaseous body so constituted as I have described, with a pressive force. Now a pressive force is an incessant and a perpetual kind of action. All opposing forces, therefore, which are to be equated with pressure, must be such as, under equal circumstances, would produce the same effects, whether these effects be estimated for a moment, for an hour, for a day, or for any time indefinitely. Hence the necessity of taking time into account; and of computing the elasticity by the sums of the collisions in a given or an indefinite time; that is, by the product of a single collision and the number of them, or by the product of a single momentum, the number of acting particles, and number of returns. Had I, therefore, omitted the factor X. objects to, I should have committed a sad error; it would have been like endeavouring to equate a single impulse with an unceasing force for an indefinite time,—a manifest impossibility.

To illustrate this, the best course is, perhaps, that which I have already pursued, p. 341 *Annals* for May. Let the perfectly hard ball A be continually solicited in the vertical direction C D by some uniform force, such as that of gravity; and when it has descended to E, and acquired the velocity a, let it be met by another perfectly hard ball B, not impelled by this gravitating force, having a contrary velocity b; so that $B\,b = A\,a$. Then the opposing momenta being equal by Prop. 5, of my theory of collision, A will begin to reascend with an equal momentum $A\,a$; and being still acted on by by the invariable gravitating force, it will continue to ascend until all its motion be destroyed. After this, it will again begin to descend, and at E will have the same momentum as before. If now it be a second time met by the ball B with the momentum $B\,b = A\,a$, it will a second time reascend and descend in precisely the same way as at the first. The circumstances of a third, fourth, &c. collision being the same, the phænomena of a third, fourth, &c. ascent and descent will be the same; and thus the effect of gravity on the one will be counteracted by the equal and uniform collisions of the other. Let f be the force of pressure or gravity, t the time of acquiring the motion $A\,a$; then $2 f t$, $4 f t$, $6 f t$ .... $2 n f t$ are the effects of gravity to be overcome by 1, 2, 3, ... n collisions; so that after the nth collision, or after a certain time T, the effect of gravity overcome is $2 n f t = n\,A\,a = n\,B\,a$. If, therefore, T be accounted from the commencement of the descent of A to the completion of the nth contact, we shall have $\frac{T - t}{2t} = (n-1)$, and $t = \frac{T}{2n-1}$. Hence $n\,B\,a = \frac{2 n f T}{2n-1}$. But if t be taken indefinitely small, the oscillations of A will not sensibly change it from a state of apparent rest, and in that case n for any given time T must become exceedingly great, so that $n\,B\,a = f T$. Putting, therefore, $T = 1$, we have $f = n\,B\,a$; that is, the force of pressure, or, which is the same, the action of the ball to support that pressure, is equal to a single momentum of the ball multiplied by the number of returns in a unity of time. Thus X. will perceive that the subject admits of a rigid mathematical proof.



X. asserts that I have, "by my own confession, assumed an hypothesis producing a result at variance with experiment." Surely X. must have been curiously mistaken. He cannot imagine I could act with such palpable absurdity. Let him look at the passage again, and he will find I have neither expressed nor implied such a variance of my theory with experiment, much less have I confessed it.

His seventh paragraph of his first paper charges me with a mathematical error. By the theorem I have given, p. 57, *Annals* for July, 1816, $E \propto \frac{T^2 N^2}{V W}$ and $\frac{T^2 N^2}{W}$, supposing the volume V constant. In this theorem, T is the true temperature, E the elasticity, N the number of particles in the air, and W the weight or specific gravity, taking V as constant. Now p being the mass of a particle, $W = p N$; and, therefore, $E \propto \frac{T^2 N^2}{W} \propto \frac{T^2 N}{p} \propto T^2 N$; for in one and the same air, as in Prop. 8, we are speaking of, p is a constant quantity. This will satisfy X. that the error does not lie on my side. Indeed from the obviousness of the thing, I was surprised he should have advanced such a charge; and still more so at his not rectifying it in his last paper.

X. says: "If the temperature be in the subduplicate ratio of the volume, that when the temperature is nothing, the volume itself is nothing." This, I grant, is a correct inference, and would have weight had I not provided against it. In the enunciation of my Prop. 7, of the first paper, I have distinctly drawn my inference on the supposition of "the particles being indefinitely small." Again, in p. 103 of the last volume, I have said: "Had Mr. Dalton applied his views of fluid expansion to gases," (that is, that the squares of the temperature are as the increments of expansion from their greatest density), he would have anticipated the general law of temperature I have given." These and other passages of the kind published before X.'s first paper appeared, clearly show that I was perfectly aware of what I was writing, and did not write without thinking. It is strange, therefore, that X. should have drawn the inference he has about "nonentities and nascent existencies." But let us take X. on his own grounds; and supposing I had not had an eye to this point of greatest density, let us see how much my determination of the real zero might err on that account. By our best experiments, steam is about 1400 times lighter than water, and nine times heavier than hydrogen. Now if hydrogen be 50 times lighter than phosgene gas, we may certainly conceive it possible for a gas to be eight or ten times lighter than hydrogen. In such a case, the volume of the body in the aeriform state would be in round numbers about 100000 times greater than in the liquid or solid, supposing the same law to hold good as in the condensation of vapour. Again, experiment teaches us that the same laws of expansion and contraction by temperature are true under one compression as under another. Let us, therefore, instead of a compression of 30 inches of mercury to the inch reduce it to three inches. Then the volumes of the body in the two states would be as the numbers 1000000 and 1. Therefore, taking this condensed volume into account, the law of temperature, instead of $\frac{1}{2}$, or ·5, ought to be ·4999995, which difference in the law being neglected, would occasion an error of $\frac{1}{10000000}$th part of each degree of Fahr.; that is, on 480°, the distance of my real zero from the melting of ice, the error would amount to less than the $\frac{1}{20000}$th of a degree. Therefore, granting to X. that gas does exist at the real zero, which, I think, he will find I have never denied nor even questioned, my determination of this point cannot be in error the twenty thousandth of a degree; and I could easily show him, if necessary, that it cannot err the twenty thousand millionth of a degree. In fact, if the experiments of Messrs. Dalton and Gay-Lussac are correct, the position of the real zero is correct. The position of this zero may be proved without having recourse to any law or theory of temperature whatever; but of this, I shall speak at another time.

Now X. denies in toto the existence of this real zero, however much experiments and theory agree, because we have never arrived at it; yet observe what he says in the eighth paragraph of his first paper: "We find by experiment that the proportion" in forming water "of two of hydrogen to one of oxygen, holds good whatever be the volumes we try, and *thence we clearly and rightly infer* that the same *must* be the case when the volumes are *infinitely small or atoms*." Who, I beg to ask X. has ever experimented with single atoms? If no one has, how comes it that X. can "clearly and rightly infer" beyond the reach of experiment, and yet another cannot? May we not from this, "*clearly and rightly infer*," that it is commonly much more easy and natural to take things for granted, and without proof, which favour one's prejudices, than to admit others, however well supported, that oppose them?

One or two curious conclusions I could draw from this paragraph of X. were I inclined; but it is much more consonant to my feelings to stop short, than to use the privilege of my own justification to draw unpleasant consequences from the opinions of one who appears disposed to be liberal. I must, however, beg to tell him, that I never "admitted that an atom may be composed of particles." A particle is composed of atoms, and may be of other particles; but an atom, which is an elementary indivisible body, cannot be composed of particles. I beg also to observe, that I have never said, I believe, "that the particles of a body in the solid move swifter than in the fluid state," though such a thing is neither impossible nor absurd.*

"He finds," says X. p. 391, *Annals* for Nov. "that within a certain range gases go on expanding nearly as the squares of a certain set of numbers. Now within the same range, the expansions are also nearly as the simple ratio of another set of numbers; that set of numbers is the common Fahrenheit temperatures; therefore, within this range little evidence is gained for or against his theory." To the first sentence I may reply, it is not a set of numbers, but a theorem, I have found. I may further observe, that this theorem I discovered in the year 1815 or 1816; and was not aware that there was a single experiment in existence to confirm it until Dr. Ure's paper on the tension of vapours appeared in the Phil. Trans. for 1818; for I have not to this day seen any of De Luc's papers. I may likewise add that I never attempted to try the truth of it myself until the fall of 1820, after I had in vain endeavoured to interest the Royal Society in the proof. This latter part can be confirmed by my cousin, Mr. W. Herapath, who knows I had no apparatus, and assisted me in making thermometers for the purpose in Aug. and Sept. 1820. Dr. Thomson can, I have no doubt, likewise recollect my asking him his opinion about June, 1820, in Queen-square, Westminster, respecting these projected experiments, and the best method of constructing thermometers for high ranges. These facts will, perhaps, satisfy the world, that I have not procured experiments, and formed a theorem to suit them; but that I first drew from my principles a theorem, in ignorance that there was any thing in existence to confirm it; then openly proposed it to the Royal Society as the test of my views; and afterwards, when they would neither try it, nor recommend it to be tried, succeeded in trying and proving it myself. Authenticated circumstances of this kind will, I have no doubt, have their weight with men of liberality, and make a due impression of the soundness of the theory I have expounded, on minds uninfluenced by interested motives in opposing it.

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* X.'s paper, *Annals of Philosophy* for Sept. p. 226.

With respect to the other part of the quotation, the best reply is that contained in p. 100, *Annals* for Aug. I have there computed three experiments, two by De Luc on water, and one out of six or seven by myself, equally consistent, on mercury. The sum of all the deviations of these experiments from my *pre-investigated* theory, is the one-tenth of a degree of Fahr.; and the sum of their differences from the old theory, sixteen degrees two-tenths. Thus, instead of the two theories agreeing, as X. says they do, nearly equally well with experiments, the one wanders 162 times further from them than the other.

These, I believe, are the principal objections which X. has advanced against my views. On most of his other observations, particularly that of capacity, which is only a suggestion, it is unnecessary for me to make any remarks. I shall, therefore, with a notice of one more of his ideas, close this paper, and take that leave of him he appears willing to take of me.

"We cannot," observes X. in his last paper, "take Mr. H.'s law of temperature as the true law, unless we are sure it holds good at *all points* in the scale; but of this we cannot be sure any further than within those limits at which experiments have been tried. How do we know that beyond those limits the law of expansion may not be modified, or some totally different law prevail?" If these arguments be admitted, we must also reject Newton's law of attraction, because we are not certain that it holds good beyond Ouranus; and we are confident it does not between very small bodies at very small distances. We must likewise reject the universality of attraction on the earth, because we are not certain "it holds good at all points." We must, moreover, for the same reason, reject the general laws of optics, electricity, magnetism, &c. and, in fine, all generalization whatever. We must descend again from general laws to insulated facts. We must destroy this beautiful system, which the reiterated efforts of the human mind have shown to exist, and have wrested from the chaos, and cleansed from the rubbish of antiquity. We must descend once more to confusion, to ignorance, to uncertainty. We must cease to admire this noble order of things, because, in all links of the chain, we are not certain of its truth. Finally, we must no longer confide in the probable continuance of phænomena whose uniformity and constancy we every day witness, because X. will not allow us to depend on the laws by which they are governed; and we must resolve into doubt and disbelief our knowledge of things whose symmetry, order, and sublimity, manifest the omniscience, and demonstrate the omnipotence, of the Deity.

I have the honour to be, dear Sir,
Your most obedient servant,
J. Herapath.

P.S. I beg leave to suggest to those who may please to support or oppose my theory, that the most effectual way of doing it is by direct experiment. There are several things I have pointed out which yet remain to be proved. The experimental confirmation or refutation of these things would be infinitely more effectual in seconding their views, and do much more good to science than all the arguments and reasoning they can employ. An excellent opportunity of verifying or refuting what I have said of capacity in p. 460, last vol. presents itself to those who have a good calorimeter. By Cor. 1, Prop. 18, if a given body at 212° Fahr. melt W quantity of ice; at 420·5° it will melt 2 W; and at 657·9°, three times W. If the theory of uniform capacities be correct, it should be 2 W at 392°, and 3 W at 572°. Should the capacities be increasing, 2 W and 3 W would come out with temperatures still lower than 392° and 572°; so that here is a fine opportunity to refute or confirm. I need hardly observe, that to be exact a quantity of the body should be used sufficient to liquefy considerable portions of the ice.

ARTICLE VII.

Account of some Vegetable Remains found in a Quarry near Bath.

By Mr. H. Woods.

(To the Editor of the *Annals of Philosophy.*)

SIR, *North Parade, Bath, Oct. 1821.*

SEARCHING for extraneous fossils in a quarry of white and blue lias at Tiverton in the neighbourhood of Bath, I discovered some *wood* in different changes of petrifaction, the appearance and situation of which I will proceed to describe as relevant to a question which I intend ultimately to ask respecting it.

The quarry consists of, first, a very thin stratum of vegetable mould; secondly, broken pieces of stone in various states of decomposition; thirdly, the first bed of white lias, about two feet thick, which, with its substratum of clay, contains a great quantity of cornua ammonis, gryphoid oysters, and several species of anomia; fourthly, the second bed, six or eight feet thick, partly blue, with its corresponding stratum of clay upon which it rests, containing but few cornua ammonis, but so numerous are its venuses, muscles, and gryphoid oysters, particularly the latter, that it may be said almost to consist of these reliquiæ of shells, agglutinated by media of sand and clay (in its clay I found a small piece of compact iron ore, and several trochitæ of the stem of the pentacrinite); and, fifthly, the third bed, which is also a mixture of white and blue stone, but with an excess of the latter, containing few petrifactions. In one part, I was told by the quarrymen, a considerable quantity of mundic or pyrites was occasionally found, and from the fissures of the stones, in addition to some small and, in most instances, imperfectly formed crystals of carbonate of lime, I picked out clay, smooth, compact, and perfectly unmixed with any other substance.

I have here described the quarry, which is, as far as it is worked, about 20 feet in depth, as I saw it, mentioning only the fossil remains which I observed and collected; but, in addition to these, the Rev. J. Townsend (in his "Character of Moses, &c."), enumerates various species of cardia, sacculi, helices, mytili, myæ, &c. but particularly siliquastra, and whole jaws of some amphibious animal. To his work I, therefore, refer for a more complete account of the lias quarries in Somersetshire, and proceed to the immediate subject of this communication.

As I was returning from the *wall* of the quarry, among a heap of the blue stones, which had been hewn into a proper size and shape for paving, I observed in one a cavity about four inches broad, and eight or ten in length, lined with an incrustation of very small brown crystals. Along this cavity, partially attached, lay a rounded body consisting of the same kind of crystals arranged longitudinally in grooves, divided at nearly regular distances by transverse septa of white crystals, the channels between which were filled up with a crumbling substance resembling charcoal, and which, like that, readily enters into combustion without flaming. The same substance seems more or less to be distributed in the interstices between the crystals, appearing thus, as if the crystallized granulations were in the direction of the fibres, or rather sap vessels of wood, and the transverse crystals, bearing a similarity to the septa so plainly observable in the timber of oak and beech.

My curiosity being excited, I returned to the quarry, in the hope of discovering more vestiges of vegetable matter, and my hope was not disappointed. *In the interstice* between two large blocks of stone, near the bottom of the third stratum, I found a larger quantity of a nearly similar substance jammed in between the two stones, but unconnected with either (the former specimen was enclosed within a solid block). The process of crystallization is not so complete in this as in the other; but although it has the same carbonized appearance, its specific gravity is greater, and it is not so readily, in fact scarcely at all inflammable, possessing a greater mixture of earthy or calcareous matter *uncrystallized.* A workman informed me that large pieces, to use his own expression, "as thick as his thigh," have been found at considerable depth in the quarry.

Mr. Townsend, in the before-quoted work, mentions charcoal being found in the great oolite and forest marble, and other authors have noticed the same phenomenon, but none, that I recollect, specify at *what depth* it has been discovered, nor, which is of still greater importance, precisely of *what substance*, whether *organic* or *inorganic*, the superstrata consisted,* the wonder being, to find the remains of wood (and I think that my specimens *are wood* is indisputable) thus situated.

My object in troubling you with so circumstantial an account is to elicit from one of your geological correspondents an explanation of the occurrence of this substance *beneath* three strata of stone, 20 feet in thickness, formed entirely of *oceanic* remains. The universal deluge was one *single* convulsion of nature. This climate is not subject to partial or secondary ones, and every appearance of the quarry evinces an uninterrupted repose during ages. The deluge might have caused the antediluvian dry land to become the bed of the postdiluvian ocean, and *vice versâ;* but this *charcoal, petrified wood, semi-coal,* or by whatever name it may be called, seems to demonstrate a convulsion *prior* to that which piled upon it an innumerable quantity of *marine* animals. A solution of this difficulty will, perhaps, be beneficial to science, and much oblige yours, &c. H. Woods.

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* Mr. Townsend also mentions *wood* mixed with *shells* in enumerating *alluvial* fossils; but that I conceive does not at all apply to the present subject.

ARTICLE VIII.

On Olefiant Gas.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Sept.* 29, 1821.

DR. HENRY, in his very interesting memoir "On the Aëriform Compounds of Charcoal and Hydrogen," read before the Royal Society in February last, and published in the *Annals* for Sept. has described a new gas obtained by heat from oil and pit coal, which possesses the property of being condensed to a liquid form by chlorine, without the agency of light, in common with olefiant gas, but differs from that compound in specific gravity, in its illuminating power, and in the properties it presents on combustion with oxygen. From the observations Dr. Henry was enabled to make on this gas, in the intervals of leisure he then enjoyed from the discharge of his professional duties, he was led to conclude that it was "either a mixture of olefiant gas with a heavier or more combustible gas or vapour, or a new gas, *sui generis*, consisting of hydrogen and charcoal in proportions which remain to be determined." A comparative examination of the different facts communicated in the memoir above referred to will be found to strengthen the former of these conclusions; but with some modification, perhaps, of the original views of this excellent and accomplished chemist. The specimen of oil gas which contained the greatest proportion of the new compound was furnished to Dr. Henry from the manufactory of Messrs. John and Philip Taylor, of London. The specific gravity of this specimen was ·906, common air being 1, and it yielded in 100 parts 38 volumes of a gas, condensible by chlorine, and 62 volumes of mixed gases, not possessing that property, being of the specific gravity of ·606. Now $\cdot906 \times 100 - \cdot606 \times 62 \div 38 = 1\cdot395$, which is the specific gravity required by the 38 volumes of condensible gas to give an aggregate weight of ·906 to the mixture. But the specific gravity of olefiant gas is ·972. It is evident, therefore, that the greater part, at least, of the above 38 volumes could not be olefiant gas, but that it consisted of some other compound, the elements of which exist in a much closer state of condensation.

The phænomena this gas exhibits with oxygen still further serve to establish that conclusion. It appears that four volumes and a half of oxygen are required for the complete combustion of one volume of the new compound, and that the gaseous product is three volumes of carbonic acid. For the saturation of one volume of olefiant gas, three volumes only of oxygen are necessary, and two volumes of carbonic acid result from their joint action. A volume and a half more of oxygen, therefore, is consumed in the present case, and an additional volume of carbonic acid produced from it, from which we may infer that the new gas contains an atom each of carbon and of hydrogen more than exists in an equal bulk of olefiant gas, and that its specific gravity, therefore, will be greater by the addition of the respective specific weights of those elements. It has been already mentioned that the specific gravity of olefiant gas is ·972, and it is well known, that this gas is formed of one atom of carbon and one atom of hydrogen. The specific gravity of vapour of carbon, as has been shown by Dr. Thomson in a former volume of the *Annals*, is ·4166, and of hydrogen ·0694; but ·4166 + ·0694 = ·486, which is only one half of the specific gravity assigned. It must be inferred, therefore, that in the constitution of olefiant gas, two volumes of vapour of carbon, and two volumes of hydrogen, are condensed into one volume—a deduction which the phenomena attending the explosion of this gas with oxygen amply confirm.

It was before stated, that three volumes of oxygen are required for the above purpose, and that two volumes of carbonic acid are the gaseous result: to explain which it is necessary to assume that two volumes of carbon are present to saturate two of the volumes of oxygen, and produce the two volumes of carbonic acid; and that there are also two volumes of hydrogen in combination to unite with the remaining volume of oxygen, and form water. In conformity with these views, the new gas will, of course, be compounded of three volumes of vapour of carbon and three volumes of hydrogen condensed into one volume; and its specific gravity, as already stated, will be the specific gravity of olefiant gas, augmented by the specific gravity of each of the additional elements of which it is composed, or ·972 + ·4166 + ·0694 = 1·458.

Proceeding upon these data, and supposing the 38 volumes of condensible gas, which formed the subject of Dr. Henry's experiments, to be "a mixture of olefiant gas with a heavier or more combustible gas," as he suggested, and that this heavier or more combustible gas is the one above described, the proportion of the two necessary to produce a specific gravity of 1·395, as deduced from his experiments, will be 100 volumes of the heavy olefiant gas, if it may be so called, and 1·49 volume (very nearly) of the light olefiant gas; for $\frac{\cdot 972\,x + 1\cdot 458}{x + 1} = 1\cdot 395$, from whence $x = \frac{\cdot 063}{\cdot 423} = \cdot 14893$.

It may be concluded from the foregoing observations, and, perhaps, satisfactorily, that the gaseous body which has been discovered by Dr. Henry among the aëriform products from oil and pit coal is not "a new gas, *sui generis*, consisting of hydrogen and charcoal in proportions which remain to be determined," but a modification of olefiant gas, constituted of the same elements as that fluid, and in the same proportions, with this only difference, that the compound atoms are *triple* instead of *double*. May we not be permitted to infer from this, that there is yet another and a simpler combination of carburetted hydrogen undiscovered, in which one atom of each of the elements is associated in the usual binary form?

ARTICLE IX.

Observations on Mr. Murray's Paper on the Decomposition of Metallic Salts by the Magnet.

(To the Editor of the *Annals of Philosophy*.)

SIR,

MR. MURRAY's paper is contained in the last number of the Philosophical Magazine, and the author prefaces his relation with saying: "I shall here take leave to select a few of the numerous experiments repeated in the course of my researches, and it would, methinks, be difficult to summon any objection to them." I beg leave to differ from Mr. Murray, and for reasons which I shall now assign.

Mr. Murray's first statement is, that "a solution of permuriate of mercury was by the magnet soon reduced into running mercury, and the supernatant fluid was not affected by the albumen of the egg."

I prepared a dilute solution of corrosive sublimate, and placed a perfectly unmagnetic steel bar in the solution, "running mercury" was immediately precipitated. Hence magnetism is not requisite to the production of the effect, and consequently it is worthy of Mr. Murray's consideration, whether "fine steel filings" unmagnetized will not be as "admirable an antidote to corrosive sublimate" as those which are magnetized. I believe their inefficacy will be equal.

Mr. Murray says: "Nitromuriate of platinum was decomposed with a brisk effervescence, distinctly audible, and with a visible spray between the eye and the light." I placed an unmagnetic steel bar in a solution of nitromuriate of platina, and the platina was precipitated with all the phenomena above described by Mr. Murray. Consequently magnetism has no share in producing the decomposition.

"Fine Dutch steel wire was selected," says Mr. Murray, "and proved to be non-magnetic. It was thrown into nitrate of silver where it remained for 14 hours without being affected, part of this was made the uniting wire between the north and south poles of two bar magnets; when it became speedily plumed with crystals of silver."

"A portion of the same wire was snapped in twain, and the magnet passed over one of the fragments, and both projected into solution of nitrate of silver. That which was magnetized reduced the silver, while the other remained inert."

I divided a dilute solution of nitrate of silver into three portions. In one I placed a steel bar hardened at the ends, but which did not attract iron filings, and consequently was not magnetized. In the two other solutions of nitrate of silver, I put magnets formed of similar bars, the north pole of one, and the south pole of the other, being immersed, their opposite poles projecting above the edges of the glasses containing the solutions; the poles were then connected by an unmagnetized steel wire. Several hours elapsed before any sensible precipitation occurred in either of the three glasses; at length a few fine brilliant flakes of metallic silver appeared in all of them, and I did not observe that they were formed sooner in one solution than the others. These flakes increased very slowly, till a certain quantity had collected, when the action increased rapidly, and an abundant precipitate of reduced silver was collected in each of the three glasses. I could perceive little or no difference in the quantity of the silver thrown down, or in any other respect, the results in the three solutions being as nearly similar as possible, excepting that from some unknown cause the unmagnetized bar was much more deeply corroded at the part in contact with the surface of the fluid. The increase of action after a certain quantity of silver had been precipitated, was probably owing to the contact of the metallic precipitate and the iron.

It is evident from these experiments that magnetism has no power whatever in modifying, increasing, or reversing the mutual action of steel and solution of nitrate of silver. Indeed it is so well an established fact, that iron precipitates silver from its solution in nitric acid, that it is quite unaccountable how it should have escaped Mr. Murray's knowledge both from reading and experiment. The following authors distinctly mention that silver is precipitated from its solution by iron. Newmann, Chemistry, page 47; Murray, vol. iii. p. 212, fourth edit.; Thenard, vol. ii. p. 314, second edit.

If magnetism were really capable of decomposing metallic salts, it would probably reverse the order of affinity, as occurs in voltaic combinations, when copper is precipitated on silver wire rendered negative by the battery. I, therefore, immersed two magnets connected by a steel bar in two separate glasses containing solutions of sulphate of zinc, the arrangement being just the same as that of the magnets in the solutions of nitrate of silver; not the least precipitate was produced in either, nor in a third solution of sulphate of zinc containing an unmagnetized bar; the only observable effect was, that the bars were all slightly tarnished.

The next experiment, related by Mr. Murray, which I shall notice is the following: "A portion of platinum wire that suffered no change in nitrate of silver in solution, was made the uniting wire between the poles of a powerful horse-shoe magnet (that supported 12 lbs. weight). When this was immersed into nitrate of silver, it soon became discoloured and acted upon."

I immersed a platina wire in a solution of nitrate of silver, the opposite the ends of the wire being connected with the two poles of a pretty strong horse-shoe magnet. After remaining about 30 hours in the solution, the surface of the platina wire was not in the slightest degree tarnished. The solution was a portion of the same as that employed in the other experiments. I conclude, therefore, that magnetism has no power whatever in influencing the action of platina on nitrate of silver.

The last statement of Mr. Murray's which I shall allude to is, that "two magnetic bars were left for two days in phosphorous acid. The acid was decomposed; the north pole of one of the bars was scarcely affected, but the north pole of the other was corroded half an inch deep, and developed the fasciculated structure described by Mr. Daniel."

I arranged some magnetic and unmagnetic bars in phosphoric acid in the same mode as described with respect to nitrate of silver. The north pole of one and the south of the other were immersed in separate glasses, and at first were not connected, but were afterwards by a smaller magnet, south and north poles being respectively in contact with north and south poles of the immersed magnets. The action of the phosphoric acid upon the magnets was not in the slightest degree increased by the contact.

The circumstances which I have now detailed, I think, justify me in concluding, that Mr. Murray's experiments are fallacious, and his inferences unwarranted by facts.

I am, Sir, your obedient servant, B. M.

Article X.

On the Properties of Peroxide of Hydrogen or Oxygenated Water. By M. Thenard. (Extracted from the last Edition of his Traité de Chimie.)

Water is combined with a large quantity of oxygen, by dissolving peroxide of barium in muriatic acid, and adding sulphuric acid to the solution. These two operations are to be several times repeated with the same liquor; then adding sulphate of silver, and at last barytes, and separating the precipitates successively by the filter. Muriatic acid readily dissolves the peroxide, and there result muriate of barytes, and weakly oxygenated water. The sulphuric acid precipitates the barytes, and liberates the muriatic acid, which then acts upon a fresh quantity of peroxide of barium, so that there is no difficulty in repeating the process several times, and there remains at length water holding more or less oxygen in solution. The mode in which the sulphate of silver acts is evident, the use of it is to separate the muriatic acid, and replace it by sulphuric; the barytes combines with the sulphuric acid, and precipitates it. When the operation is performed with pure materials, and in proper proportions, it is evident that the last result is entirely oxygenated water: it is then to be put into a glass vessel with a foot, and this placed in a large capsule, two-thirds filled with concentrated sulphuric acid, the apparatus is to be put under the receiver of an air-pump, and the air exhausted. The pure water evaporates much more readily than the oxygenated water, so that in two days it will probably contain 250 times its volume of oxygen; and when the solution contains 475 times its volume of oxygen at the temperature of 57°, no further concentration takes place by keeping it longer in vacuo.*

Physical Properties of the Peroxide of Hydrogen.

The peroxide of hydrogen is fluid and colourless as water. It is inodorous, or at least it is so nearly so, that few persons can discover any smell. It gradually destroys the colour of litmus and turmeric paper, and makes them quite white. It acts upon the epidermis very readily, sometimes suddenly, whitens it, and occasions prickings, which continue for a longer or shorter period, according to the nature of the individual and the thickness of the portion of liquor applied; if it be too thick or be renewed, the skin itself is attacked and destroyed. Applied to the tongue, it whitens and pricks it, thickens the saliva, and produces a sensation which it is difficult to describe, but which resembles that of certain metallic solutions. Its tension is extremely weak, much weaker than that of water: this is the reason why oxygenated water, at common temperatures, is concentrated in vacuo by the intervention of an absorbing body such as sulphuric acid. This also is the reason why the evaporation in this case becomes gradually slower, so that at the end it is extremely slow; still, however, it always takes place, for it finishes by the whole of the liquor disappearing; and this occurs without the production of any gas, which shows that the peroxide of hydrogen is vaporized without decomposition.

I tried, but ineffectually, to solidify the peroxide of hydrogen. Exposed to a low temperature for three quarters of an hour, it remained liquid; when also water which contains only 30 or 40 times its volume of oxygen, is subjected to a temperature of 12°, the part which remains fluid is much more oxygenated than that which freezes. It is even probable that if the latter contains any oxygen, that it is derived from a certain quantity of interposed water.

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* For the precautions requisite to be observed in preparing oxygenated water, the reader is referred to M. Thenard's Traité de Chimie, tom. i. p. 563.

I thought at first that I might employ this process to concentrate the oxygenated water, especially by taking care to break the ice and to press it strongly in linen: it did not succeed; the ice even after compression retained too much oxygen to be rejected.

One of the properties of peroxide of hydrogen which I endeavoured more particularly to establish, is its density; this I found to be 1·452. It is, therefore, evident, that the peroxide is much more dense than water, and it is not necessary to take its specific gravity to be convinced of this; it is sufficient to pour it into water, for although it is very soluble, it flows through it like a syrup.

Of the Action of different Bodies upon the Peroxide of Hydrogen.

There are some bodies which have no action upon peroxide of hydrogen; others render it more fixed, while some decompose it, and combine with a part of its oxygen; but it is particularly worthy of remark, that a considerable number decompose it at common temperatures without uniting either to the water or to the oxygen gas which results: sometimes even this decomposition occurs with a sort of detonation, owing to the sudden disengagement of the gas. In this case, the temperature is so far from being reduced, as might be supposed on account of the oxygen passing to the gaseous state, that it is so much raised as to produce light. Sometimes also the body during its decomposition of the peroxide, is itself decomposed; such is, for example, the oxide of silver, which immediately upon coming into contact with the peroxide, even largely diluted with water, disengages all the oxygen, and is itself reduced.

Of the Action of Imponderable Bodies.

Heat quickly decomposes the peroxide of hydrogen; but the decomposition takes place more slowly as it proceeds. The water, in proportion as it is liberated, undoubtedly combines with the undecomposed portion, and renders it more fixed. This may be learned by the following experiments:

Put some peroxide of hydrogen into a small glass tube, heat it gradually from 55° to 212°, by placing the tube in water, it will be seen that the decomposition will be quite sensible at 68°; it occurs with greater ebullition, if the peroxide is subjected immediately to 212°; the experiment would be dangerous in a vessel with a narrow neck, with eight grains of the peroxide. Nevertheless, when thrown upon a red hot metal plate, it does not detonate.

Let this experiment be repeated, after having so diluted the peroxide, that it shall contain only seven or eight times its volume of oxygen, the disengagement of the gas will not be perceptible, even at 120°, but it becomes so soon afterwards, and goes on increasing until it ceases. From this period, the liquor contains no gas, and consequently will not effervesce with oxide of manganese.

All other circumstances being equal, peroxide of hydrogen suffers no more alteration by exposure to light than in darkness. In both cases, small bubbles are disengaged from time to time, and it finishes at the expiration of some months, even at common temperatures, with being for the most part deoxidized. This deoxidizement, which probably depends upon many causes, appears to me to be principally produced by some particles of matter which the peroxide retains. To preserve it as much as possible, it must be surrounded with ice.

When the peroxide is subjected to the action of the voltaic pile in the same way that water usually is, similar results are in both cases produced, excepting that with the peroxide, the disengagement of oxygen gas is much greater. I ought to observe, however, that I have not collected the gases to examine them.

Of the Action of the Metals at Common Temperatures.

In general the metals tend to decompose the peroxide of hydrogen, and to restore it to the state of protoxide or water. I know only four which do not sensibly possess this property; iron, tin, antimony, and tellurium. The most oxidizable are oxidized, and at the same time produce a disengagement of oxygen. The others, on the contrary, retain their metallic state, so that all the oxygen with which the water combines to become peroxide is liberated.

In order to effect the decomposition readily, it is indispensably necessary that the metallic matter should be finely divided. Any metal which in the state of fine powder readily disengages the oxygen of the peroxide, effects it very slowly if the powder be coarse, and still more so if it be in mass.

The same phenomena occur even when the peroxide is diluted with water, excepting that they are less distinct, and continue longer. This will appear from the examination which I am going to state with respect to the action of metals upon the diluted peroxide.

The experiments were all performed in the same way. The liquid was first put with a small pipe into a little glass tube closed at one end, after which the metal was introduced. The quantity of peroxide employed in each experiment amounted only to a few drops; when diluted with water, a larger quantity was employed. The action was considered as complete, when no more gas was evolved; and this was rendered certain by the addition of a small quantity of oxide of manganese. All the metals were tried in this manner, excepting uranium, titanium, cerium, barium, strontium, calcium, lithium, and the metals of the earths.

Of the Metals which decompose the Peroxide of Hydrogen, and disengage the Oxygen without undergoing any Change.

Silver, finely divided, procured by the recent decomposition of nitrate of silver by copper, and pure peroxide of hydrogen. Sudden and violent action, the extrication of heat so great, that the tube became burning hot; the silver retained its metallic state, and all the oxygen was instantly disengaged.

Silver, finely divided, and peroxide containing nine times its volume of oxygen. Sudden and brisk effervescence, no sensible heat: the silver was not oxidized; the action was soon over, and all the oxygen was disengaged. The tube is not heated unless the peroxide contains at least 30 times its volume of oxygen.

Silver precipitated from the solution of nitrate of silver by copper, but the parts of which were become less finely divided by drying. Action upon the peroxide much weaker than with the finely divided silver of the two preceding experiments.

Silver in filings. Action much weaker than the last.

Silver in mass. Action extremely weak compared to that with divided silver.

Platina in fine powder, prepared from the ammoniaco-muriate, calcined with common salt, and pure peroxide of hydrogen. Phenomena similar to those with silver; the action, perhaps, a little stronger. I do not conclude from this, that the platina itself acts more upon the peroxide than silver; for in order to ascertain this, the state of division of the metallic particles, which so much influences their action, must be equal.

Platina in fine powder, and peroxide containing nine times its volume of oxygen. Phenomena similar to those with silver.

Platina in filings and in mass. The same action upon the peroxide as with silver in filings and in mass.

Gold, finely divided, procured from the decomposition of muriate of gold by sulphate of iron. The same action upon the pure and diluted peroxide as with silver and platina, provided the liquid be not sensibly acid.

Gold in filings and in mass. The same action upon the peroxide as with silver in filings and in mass.

Osmium in black powder and pure peroxide. Action more violent than with the preceding metals, which may depend upon the metal being more finely divided: in other respects, the phenomena were similar; the same effects, except as to intensity, with osmium and diluted peroxide as with platina and silver.

Palladium in powder, prepared by calcining ammoniaco-muriate of palladium, and pure peroxide. Ready and very lively action, but less so than that of platina, silver, gold, and osmium; great extrication of heat. All the oxygen was disengaged almost as soon as the action occurred; the metal did not appear to be oxidized. If the peroxide were sensibly acid, it acted much less readily.

Palladium in powder, and peroxide containing only nine volumes of oxygen. The same phenomena as with silver, excepting that the disengagement of oxygen was rather less rapid.

Rhodium in powder, prepared by calcining the ammoniaco-muriate of rhodium, with pure and diluted peroxide. The action of this metal is nearly the same as that of palladium, excepting that the presence of a little acid did not retard it so much.

Lead reduced to fine filings and pure peroxide. Action at first slow, but which gradually increases, and finishes in a few minutes, becoming extremely strong, and exciting much heat. All the oxygen is disengaged, and I do not think that the lead is oxidized.

Lead reduced to fine filings, and peroxide containing nine volumes of oxygen. Action at first weak, gradually becoming stronger, and then the bubbles of oxygen are rapidly liberated, and raise the metallic particles. Is there not a little oxide formed, which, it will be hereafter seen, readily decomposes oxygenated water? It is certain, that at the expiration of an hour, no oxygen remains in the liquor.

Bismuth, well powdered, and pure peroxide. The same phenomena as with lead.

Bismuth, well powdered, and liquor containing only nine volumes of oxygen. The action is extremely slow. Bubbles are only occasionally given out; but at the end of some hours, the liquor was always deoxidized. The metal did not appear to be oxygenated.

Mercury and pure peroxide. The same phenomena as with lead and bismuth, provided the solution be not acid; when it contains a little sulphuric acid, there is also formed a red substance, which is probably a subsulphate.

Mercury and peroxide containing only nine volumes of oxygen. Very evident disengagement of gas, especially when the solution is rather alkaline than acid: the mercury is not oxidized: one drop of a very weak acid is sufficient to stop the disengagement.

Cobalt, nickel, cadmium, copper. Very weak action.

Of the Metals which decompose the Peroxide of Hydrogen, absorbing Part of the Oxygen, and disengaging the remainder.

Arsenic in powder and pure peroxide. Sudden and most violent action; flame produced by the combustion of the arsenic, which, acidifying, prevents the whole of the oxygen from being disengaged or absorbed, at least instantaneously; consequently very great disengagement of heat. When the peroxide is in excess, all the arsenic becomes acid, and is dissolved.

Arsenic in powder and peroxide containing only one-ninth of its volume of oxygen. No effervescence; the liquor becomes immediately acid. This acid rendering the peroxide more fixed, it remains for a long time more or less oxidized.

Molybden reduced to powder and pure peroxide. Very violent action; combustion of the metal with light; great extrication of heat; production of a very soluble acid, the taste of which is rather strong, and gives a yellow colour to the water. All the molybden disappears when the peroxide is in excess.

Molybden reduced to powder, and peroxide containing only nine volumes of oxygen. Sudden brisk effervescence; production of acid; absorption or disengagement of all the oxygen: at the end of 15 hours, the liquor was of a superb blue colour.

Tungsten, chrome, and pure peroxide. The action weak at first; and with the tungsten only after some time it becomes violent.

Potassium and pure peroxide. Sudden and violent action; vivid combustion; disengagement of oxygen, and formation of alkali: the experiment ought not to be made in a narrow tube, for sometimes explosion occurs.

Sodium and pure peroxide. The same phenomena as with potassium.

Manganese and pure peroxide. The metal, in the form of small globules, produces brisk effervescence, and deoxidizes the liquor readily. May it not be imagined that it is first oxidized, and that it is the oxide which expels the oxygen? Yet the globules did not appear to be altered. In powder it acts still more strongly, becoming very soon violent: at the same time that the oxygen is disengaged, great heat is excited.

Manganese and peroxide containing only nine times its volume of oxygen. Brisk and sudden effervescence; no heat; complete deoxidizement of the liquor in a short time.

(*Zinc.*) Action very weak.

Iron, tin, antimony, tellurium. No, or scarcely any, action at all, even with the concentrated liquor.

Action of the simple non-metallic Combustibles.

Among the simple non-metallic combustible bodies, there are only selenium and charcoal, which act upon peroxide of hydrogen in a marked manner.

Selenium in powder and pure peroxide. Sudden and very violent action; disengagement of great heat without light; complete acidification of the selenium, which, owing to this, immediately dissolves.

Selenium and peroxide containing only nine times its volume of oxygen. No heat. Occasional bubbles are disengaged; but the liquor is acidified in a few minutes.

Charcoal in fine powder and pure peroxide. Sudden and very brisk action; production of very considerable heat; disengagement of all the oxygen without the formation of any carbonic acid.

Charcoal in fine powder and peroxide containing only nine times its volume of oxygen. Brisk effervescence without heat; all the oxygen is disengaged without the production of carbonic acid. Pass a certain quantity of the liquor up an inverted tube containing mercury, then introduce some well powdered charcoal. It will be found that the gas which is readily evolved from the liquor is merely oxygen, and that it will be deoxidized in a very short time.

Lamp Black. No action, unquestionably because the liquor does not moisten it.

Action upon Metallic Sulphurets at Common Temperatures.

The greater number of the metallic sulphurets which I have tried have a very marked action upon the peroxide of hydrogen. Very often this action is violent, and accompanied with much heat when the liquor is concentrated. Moreover, whether it be diluted with water or concentrated, there almost always results a sulphate, and a more or less sensible disengagement of oxygen. This occurs with the sulphurets of copper, antimony, lead, and iron: they are scarcely brought into contact before they are converted with effervescence into sulphates.

The sulphurets of arsenic and of molybeden act with more violence than the preceding upon the concentrated peroxide; heat and light are produced; but no sulphate is formed; the arsenic is acidified, and the sulphur remains almost unacted upon. The sulphurets of bismuth and of tin act very feebly, even upon the peroxide in the most concentrated state; the sulphurets of silver and of mercury (cinnabar) have no action at all.

Action of Metallic Oxides at Common Temperatures.

In general, metallic oxides tend to restore the peroxide of hydrogen to the state of protoxide or water. Some of them produce this effect by becoming more oxidized; others without alteration, but disengaging all the oxygen in the gaseous form, which water absorbs to become peroxide. Some again disengage the oxygen, and are themselves reduced; but few exert no action at all.

The decomposing power of the oxides varies much. Several expel the oxygen so suddenly from the liquor, that a kind of explosion occurs, and then much light and heat are evolved. The action of others, on the contrary, is slow, occasioning but slight effervescence, and no sensible heat.

Of the Oxides which absorb the Oxygen of the Peroxide, and restore it to the State of Protoxide or Water.

These oxides are barytes, strontian, lime, oxide of zinc, oxide and peroxide of copper, oxide of nickel, the protoxides of manganese, iron, tin, cobalt, oxide of arsenic, and probably several others, is requisite that the metallic oxide should be moist or in solution: otherwise the oxygen would be disengaged, or would remain in combination. It is moreover evident that in proportion as the new oxide is produced, it is possible that it may expel a portion of the oxygen from the liquor, so that the action may become complicated.

Barytes. When barytes water is poured into concentrated or diluted peroxide, a great number of brilliant scales are precipitated; these are merely hydrate of peroxide of barium, but if barytes reduced to powder be used instead of barytes water, with slightly diluted peroxide of barium, a violent extrication of oxygen gas takes place, and much heat is excited. This heat may be derived from the absorption of the water of the peroxide by the barytes. As to the disengagement of the oxygen, it may be attributed to the heat produced by the absorption of water, and the formation of a small quantity of peroxide of barium: hydrate of barytes possesses the power of evolving oxygen from the peroxide of hydrogen in all cases.

Strontian. Strontian presents the same appearances with the peroxide as barytes does.

Lime. This base also produces with the peroxide of hydrogen, phenomena analogous to those which have been mentioned with the two preceding bases.

Hydrate of Copper. This hydrate, when mixed with the peroxide of hydrogen, becomes immediately a new oxide of an ochre-yellow colour, and it rapidly evolves the oxygen of the peroxide which remained undecomposed. When the peroxide is concentrated, the action is vivid, there is disengagement of heat, and it requires much to convert all the oxide of copper into peroxide. In order that the peroxidation may take place, it is not only requisite that the peroxide of hydrogen should be diluted with water, but other circumstances hereafter to be mentioned must be attended to.

Calcined Peroxide of Copper. In this state the oxide of copper cannot of course combine with more oxygen; it produces a very evident effervescence of oxygen gas when put into peroxide of hydrogen.

Hydrate of Zinc. The same as copper: this oxide becomes a peroxide with oxygenated water, so that very little oxygen is evolved.

Oxide of Zinc by Calcination. More converted into peroxide than in the former case; the evolution of oxygen gas is extremely slight.

Hydrate of Nickel. This is another oxide, which, with the peroxide of hydrogen, probably forms a new oxide; it also occasions a slight disengagement of oxygen.

Oxide of Nickel by Calcination. Very evident effervescence of oxygen from the peroxide of hydrogen.

Protoxide of manganese, iron, tin, cobalt. These protoxides, when in the state of hydrates, are converted into peroxides in the same way as those already described. When oxygenated water is poured upon these hydrates recently precipitated by potash from their solution in acids, they are immediately peroxidized. The peroxides of manganese and cobalt will afterwards act upon the undecomposed peroxide, causing the rapid expulsion of its oxygen in the state of gas; the action of the peroxide of iron is not very strong, and that of tin produces no sensible effect.

Oxide of Arsenic becomes acidified.

Of the Oxides which expel Oxygen from the Peroxide of Hydrogen without being Peroxidized or Deoxidized.

There are a considerable number of oxides which possess this property; they will be described as nearly as possible in the order of their power of decomposing.

Native peroxide of manganese in fine powder, with concentrated peroxide of hydrogen. Sudden and very violent action; the heat occasioned so great as to make the tube burning hot; the deoxidation of the peroxide of hydrogen instantaneous and complete.

The same oxide of manganese with peroxide containing only nine volumes of oxygen. Very brisk and sudden effervescence; all the oxygen disengaged in a very short time from the oxygenated water.

Very finely divided peroxide of manganese, obtained by adding oxygenated water to a solution of manganese, and decomposing the solution by potash. The action of this oxide is stronger than that of the native oxide; and when the experiment is performed with the concentrated oxygenated water, it takes place with a kind of explosion.

Peroxide of cobalt in powder. This produces the same effects with the concentrated peroxide of hydrogen as the native peroxide of manganese does.

Massicot in powder and highly concentrated peroxide of hydrogen. Violent action, great heat; disengagement of all the oxygen in a few minutes.

Minium and peroxide of lead. These two oxides act also very strongly upon the peroxide of hydrogen; the action of the peroxide is extremely violent, and it becomes protoxide.

Hydrate of peroxide of iron, and concentrated peroxide of hydrogen. Action soon becoming very strong; great heat, and complete deoxidizement of the liquor in a very short time.

Hydrate of peroxide of iron, and liquor containing only one-ninth of its volume of oxygen. Very sudden effervescence, but not brisk; so that the deoxidizement requires some hours for its completion.

Oxide of iron, from the decomposition of water by hot iron. Weak action upon the peroxide both concentrated and diluted. Fifteen hours were not nearly sufficient to complete the deoxidizement of the liquor; for it was found after this time nearly unaltered.

Oxide of nickel, peroxide of copper, oxide of bismuth. The action of true oxides upon the concentrated liquor is not very strong, but it is sufficient to evolve all the oxygen in the space of a few hours, and in 15 hours they evolve it from the peroxide containing only one-ninth of its volume of oxygen.

Potash, soda. Strong action even when they are dissolved in water upon the concentrated peroxide of hydrogen; rather rapid evolution of oxygen; very soon perfect deoxidizement. When the peroxide of hydrogen is diluted with water, the decomposition takes place less rapidly, but eventually all the oxygen is expelled.

Gelatinous magnesia, and highly concentrated peroxide of hydrogen. Very evident evolution of oxygen gas which gradually subsides before the total deoxidizement.

Gelatinous magnesia, and liquor containing nine times its volume of oxygen. Rather brisk effervescence, which gradually subsides before the deoxidizement is complete. It appears, however, to evolve proportionally more oxygen when the liquor is dilute than when it is concentrated.

Magnesia in powder. The action is weaker than when in the gelatinous state.

Hydrate of barytes, strontian, and lime. But little action.

Oxide of uranium, procured by decomposing sulphate of uranium with potash. Still less action than the last oxides.

Oxide of titanium in powder, sublimed oxide of zinc, oxide of cerium. Weak effervescence. At the end of 30 hours the liquor was scarcely deoxidized.

Of the Oxides which evolve the Oxygen of the Peroxide of Hydrogen, and which at the same time lose their own either partially or totally.

These oxides are the oxides of silver, mercury, deutoxide and peroxide of lead, of gold, platina, and probably iridium, palladium, and rhodium.

Oxide of silver. Of all oxides this appears to have most action upon the peroxide of hydrogen; it immediately expels its oxygen, and this occurs so rapidly, that explosion may happen when the peroxide is concentrated: moreover, the heat produced is such that luminous spots are perceived when the experiment is performed in the dark. Under these circumstances, it is not extraordinary that the oxide of silver should be reduced: the experiment should not be made in a narrow tube.

The action is very strong, even when the peroxide of hydrogen is diluted in water. In fact, oxide of silver occasions very evident and sudden effervescence in water which contains only a fiftieth of its volume of oxygen; so that, when a tube is filled with mercury, and inverted, and water containing 12 times its volume of oxygen is passed up into it, oxide of silver afterwards thrown up sinks the mercury so suddenly that the eye follows it with difficulty. In this case, there is no sensible production of heat, and yet the oxide of silver is reduced. This oxide is reduced even in the most diluted peroxide of hydrogen, so that it must not be conceived that the expulsion of the oxygen from the metal is not the effect of temperature; it may happen that at the moment of the action of the oxide of silver upon the peroxide of hydrogen, the particles which act upon each other are much heated, and that their number being very small compared with the liquor, they are incapable of raising its temperature half a degree.

Peroxide of lead in powder. The action of this oxide upon the peroxide of hydrogen is nearly as strong as that of the oxide of silver, and the results are similar, excepting that the peroxide of lead is not reduced, but becomes merely yellow protoxide in the concentrated liquor. I doubt whether it undergoes similar deoxidizement in the diluted liquor.

Minium and peroxide of hydrogen. The same phenomena as with the peroxide, excepting that the action, which is less rapid, takes place without the evolution of light, and with less extrication of heat.

Hydrated peroxide of mercury, and peroxide of hydrogen. The hydrate of mercury previously moistened with water was put upon blotting paper, and the trial was then made in the usual way. In a moment, the yellow colour of the oxide became red, effervescence occurred, and soon became violent; there was then great extrication of heat, the mercurial oxide was reduced, and the liquor completely deoxidized.

Hydrate of peroxide of mercury, and liquor containing only nine volumes of oxygen. Very moderate effervescence; no sensible heat; the oxide reduced in 24 hours; complete deoxidizement of the liquor also, provided the peroxide of mercury is in excess.

Peroxide of mercury by heat in fine powder. This oxide in powder was of a greenish ochre yellow colour; when put into the concentrated peroxide of hydrogen, it became red, like the hydrate, and acted like it, but less quickly; the action always finished violently, the disengagement of heat being very great, and the oxide reduced. Its action upon the diluted liquor is weak.

Brown oxide of gold in powder, and highly concentrated peroxide. Action sudden; violent; great extrication of heat; reduction of the gold; complete deoxidizement of the liquor.

Oxide of gold, and liquor containing only nine volumes of oxygen. Sudden, brisk effervescence; no heat; the gold reduced; and the liquor deoxidized in a short time.

Oxide of platina in powder, obtained by boiling muriate of platina with soda. Similar action upon concentrated and diluted peroxide of hydrogen as the oxide of gold.

Oxide of osmium, procured by calcining osmium with chlorate of potash, and highly concentrated peroxide of hydrogen. No sensible action; but as soon as a small quantity of potash is added, great effervescence; much heat; and the clear colourless liquor becomes of a dark brown. It is uncertain whether the oxide of osmium is reduced. The peroxide diluted with water acts similarly, excepting with less intensity.

Of the oxides which do not act sensibly, if at all, upon the peroxide of hydrogen. These are alumina, silica, oxide of chrome, peroxide of tin, protoxide and peroxide of antimony and tungstic acid.

Several other oxides are undoubtedly similarly circumstanced, but having had no opportunity of trying them, I cannot speak with any certainty.

Article XI.

Astronomical Observations, 1821.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Nov. 20. Emersion of Jupiter's first satellite	9h	24′	34″	Mean Time at Bushey.
	9	25	55	Mean Time at Greenwich.
Nov. 27. Emersion of Jupiter's first satellite	11	20	19	Mean Time at Bushey.
	11	21	40	Mean Time at Greenwich.
Nov. 29. Emersion of Jupiter's first satellite	5	49	13	Mean Time at Bushey.
	5	50	43	Mean Time at Greenwich.
Nov. 29. Emersion of Jupiter's third satellite	6	42	08	Mean Time at Bushey.
	6	43	38	Mean Time at Greenwich.
Dec. 6. Emersion of Jupiter's first satellite	7	45	20	Mean Time at Bushey.
	7	46	41	Mean Time at Greenwich.
Dec. 6. Immersion of Jupiter's third satellite	8	35	21	Mean Time at Bushey.
	8	36	42	Mean Time at Greenwich.

Article XII.

The Mean Places of 46 *Greenwich Stars, reduced to Jan.* 1, 1822, *from the Catalogue published in the Nautical Almanac for* 1823. By James South, Esq. FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Blackman-street, Dec.* 25, 1821.

In the Quarterly Journal of Science published in January and July last, Corrections in Right Ascension of the 36 principal Fixed Stars, were published by me for every day of the present year, and it was my intention to have continued them annually had not the Astronomical Society taken up the matter upon a more extensive scale; and under the idea that the first results of its labours would have appeared before the public in time sufficient to render the prosecution of my plan almost, if not altogether useless, that leisure which must have been employed by me to have had the computations ready against the present period, has been otherwise disposed of. Should, however, fresh delays long postpone the publication of the Society's tables, I shall feel it my duty to resume the task, unless, in the mean time, some one should anticipate me.

Unable, therefore, at present to give the corrections, I must content myself with offering a catalogue of the mean places of the 46 stars reduced to January 1, 1822, and I am the more anxious to do this, in consequence of one which has appeared in the Nautical Almanac for 1824; of this production it is needless for me to say any thing, except that it is inaccurate and unsatisfactory; inaccurate, as far as the north polar distances of its stars are concerned; and unsatisfactory, inasmuch as the long continued habit of giving the right ascensions to hundredths of seconds, has been abandoned. While, however, in common with others, I lament that such a catalogue should have found its way to the pages of a book which, in accuracy and precision, should be surpassed by no one issuing from the press, I cannot consent to withdraw my confidence altogether from preceding catalogues, and go abroad in search of better; thinking, as I do, that although unforeseen circumstances may have conspired to render one objectionable, still all should not be placed in the like condemnation. Under this impression, let me urge upon my fellow labourers in the same pursuit, the propriety of adhering to their own Greenwich catalogue published in the Nautical Almanac for 1823; so will their observations tally with each other's, and also with those made at our own great national establishment, with instruments which are not less the pride and glory of Great Britain, than they are the envy and admiration of the world.

J. South.

The Mean Places of 46 Greenwich Stars. Reduced to Jan. 1, 1822.

Stars' names.	Right ascension.			N. P. D.			Declination.			
γ Pegasi	0	4	5·00	75	48	19·75	14	11	40·25	N
α Cassiopeiæ.	0	30	27·61	34	26	24·48	55	33	35·52	N
Polaris	0	57	30·08	1	38	26·67	88	21	33·33	N
α Arietis	1	57	9·71	67	22	59·89	22	37	0·11	N
α Ceti	2	52	59·13	86	36	48·69	3	23	11·31	N
α Persei.	3	11	39·65	40	46	50·87	49	13	9·13	N
Aldebaran.	4	25	43·14	73	51	23·77	16	8	36·23	N
Capella	5	3	33·41	44	11	39·35	45	48	20·65	N
Rigel.	5	5	59·32	98	24	50·01	8	24	50·01	S
β Tauri.	5	15	2·97	61	33	9·23	28	26	50·77	N
α Orionis	5	45	32·42	82	38	3·30	7	21	56·70	N
Sirius.	6	37	18·12	106	28	39·39	16	28	39·39	S
Castor	7	23	13·81	57	43	50·43	32	16	9·57	N
Procyon	7	29	58·97	84	19	32·77	5	40	27·23	N
Pollux	7	34	24·85	61	33	8·45	28	26	51·55	N
α Hydræ	9	18	50·51	97	53	27·57	7	53	27·57	S
Regulus.	9	58	53·16	77	9	58·28	12	50	1·72	N
α Ursæ Maj.	10	52	39·65	27	17	25·20	62	42	34·80	N
β Leonis.	11	39	58·59	74	25	58·01	15	34	1·99	N
β Virginis	11	41	25·59	87	13	54·66	2	46	5·34	N
γ Ursæ Maj.	11	44	25·53	35	18	55·25	54	41	4·75	N
Polaris, S. P.	12	57	30·03	1	38	26·67	88	21	33·33	N
Spica Virg.	13	15	49·74	100	13	41·18	10	13	41·18	S
η Ursæ Maj.	13	40	31·19	39	47	41·61	50	12	18·39	N
Arcturus.	14	7	32·90	69	53	9·87	20	6	50·13	N
1 α Libræ.	14	40	51·61	105	14	56·00	15	14	56·00	S
2 α Libræ	14	41	3·05	105	17	40·13	15	17	40·13	S
β Ursæ Minor	14	51	19·72	15	7	1·23	74	52	58·77	N
α Coronæ Bor.	15	27	9·40	62	40	47·76	27	19	12·24	N
α Serpentis	15	35	30·55	83	0	24·79	6	59	35·21	N
Antares	16	18	30·55	116	1	33·34	26	1	33·34	S
α Herculis.	17	6	32·29	75	23	54·55	14	36	5·45	N
α Ophinchi.	17	26	40·70	77	18	6·75	12	41	53·25	N
γ Draconis	17	52	28·65	38	29	9·85	51	30	50·15	N
α Lyræ.	18	30	54·95	51	22	33·45	38	37	26·55	N
γ Aquilæ	19	37	47·08	79	48	45·46	10	11	14·54	N
α Aquilæ.	19	42	6·00	81	35	37·27	8	24	22·73	N
β Aquilæ	19	46	34·31	84	1	46·99	5	58	13·01	N
1 α Capricorni	20	7	46·56	103	2	59·00	13	2	59·00	S
2 α Capricorni	20	8	10·31	103	5	15·75	13	5	15·75	S
α Cygni	20	35	22·14	45	21	3·33	44	38	56·67	N
α Cephei	21	14	19·55	28	9	58·45	61	50	1·55	N
β Cephei	21	26	19·58	20	13	9·24	69	46	50·76	N
α Aquarii.	21	56	38·44	91	10	45·81	1	10	45·81	S
Fomalhaut	22	47	47·64	120	33	47·60	30	33	47·60	S
α Pegasi	22	55	54·20	75	44	57·10	14	15	2·90	N
α Andromedæ	23	59	12·52	61	53	30·33	28	6	29·67	N

ARTICLE XLII.

Observations on Mr. Schoolcraft's "Account of the Native Copper on the Southern Shore of Lake Superior, with Historical Citations and Miscellaneous Remarks, &c." By John Taylor, Esq. MGS. Mining Engineer.

(To the Editor of the *Annals of Philosophy.*)

SIR,

It appears from the information detailed by Mr. Schoolcraft, that specimens of native copper, varying in weight from a few pounds, have been at various times found on the shore of Lake Superior. Mr. S. says:

"The first appearances of copper are seen on the head of the portage across Keweena point, two hundred and seventy miles beyond the Sault de St. Marie, where the pebbles along the shore of the lake contain native copper disseminated in particles varying in size from a grain of sand to a lump of two pounds weight. Many of the detached stones at this place are also coloured green by the carbonate of copper, and the rock strata in the vicinity exhibit traces of the same ore. These indications continue to the river Ontonagon, which has long been noted for the large masses of native copper found upon its banks, and about the contiguous country. This river (*called Donagon on Mellish's Map*) is one of the largest of thirty tributaries which flow into the lake between Point Iroquois and the Fond du Lac. It originates in a district of mountainous country intermediate between the Mississippi river and the lakes Huron and Superior, and after running in a northern direction for one hundred and twenty miles, enters the latter at the distance of fifty one miles west of Point Keweena, in north latitude 46° 52′ 2″ according to the observations of Capt. Douglass. It is connected by portages with the Menomonie river of Green Bay, and with the Chippeway river of the Mississippi, routes of communication occasionally travelled by the Indians in canoes. At its mouth there is a village of Chippeway Indians of sixteen families, who subsist chiefly on the fish (sturgeon) taken in the river; and whose location, independently of that circumstance, does not appear to unite the ordinary advantages of Indian villages in that region. A strip of alluvial land of a sandy character extends from the lake up the river three or four leagues, where it is succeeded by high broken hills of a sterile aspect and covered chiefly by a growth of pine, hemlock, and spruce. Among these hills, which may be considered as lateral spurs of the Porcupine mountains, the Copper Mines, so called, are situated, at the distance of thirty two miles from the lake, and in the centre of a region characterized by its wild, rugged, and forbidding appearance. The large mass of native copper reposes on the west bank of the river, at the water's edge, and at the foot of a very elevated bank of alluvion, the face of which appears, at some former period, to have slipped into the river, carrying with it the mass of copper, together with detached blocks of granite, hornblende, and other bodies peculiar to the soil of that place. The copper, which is in a pure and malleable state, lies in connexion with serpentine rock; the face of which it almost completely overlays, and is also disseminated in masses and grains throughout the substance of the rock. The surface of the metal, unlike most oxydable metals which have suffered a long exposure to the atmosphere, presents a metallic brilliancy; which is attributable either to alloy of the precious metals, or to the action of the river, which during its semi-annual floods carries down large quantities of sand and other alluvial matter, that may serve to abrade its surface, and keep it bright. The shape of the rock is very irregular—its greatest length is three feet eight inches—its greatest breadth three feet four inches, and it may altogether contain eleven cubic feet. In size, it considerably exceeds the great mass of native iron found some years ago upon the banks of Red River in Louisiana, and now deposited among the collections of the New York Historical Society; but, on account of the admixture of rocky matter, is inferior in weight. Henry, who visited it in 1766, estimated its weight at five tons. But, after examining it with scrupulous attention, I have computed the weight of *metallic copper* in the rock at twenty two hundred pounds. The quantity may, however, have been much diminished since its first discovery, and the marks of chissels and axes upon it, with the broken tools lying around, prove that portions have been cut off, and carried away. The author just quoted observes, 'that such was its pure and malleable state, that with an axe he was able to cut off a portion weighing a hundred pounds.' Notwithstanding this reduction it may still be considered one of the largest and most remarkable bodies of native copper upon the globe, and is, so far as my reading extends, exceeded only by a specimen found in a valley in Brazil, weighing 2666 Portuguese pounds."

After various details as to circumstances under which the copper has been found, the author proceeds:

"From all the facts which I have been able to collect on lake Superior, and after a deliberation upon them since my return, I have drawn the following conclusions:—

"1st. That the alluvial soil along the banks of the Ontonagon river, extending to its source, and embracing the contiguous region which gives origin to the Menomonie river of Green Bay, and to the Ousconsing, Chippeway, and St. Croix rivers of the Mississippi, contains very frequent, and some most extraordinary imbedded masses of native copper; but that no body of it, which is sufficiently extensive to become the object of profitable mining operations, is to be found at any particular place. This conclusion is supported by the facts already adduced, and so far as theoretical aids can be relied upon, by an application of those facts to the theories of mining. A further extent of country might have been embraced along the shore of lake Superior, but the same remark appears applicable to it.

"2d. That a mineralogical survey of the rock formations skirting the Ontonagon, including the district of country above alluded to, would result in the discovery of very valuable mines of the sulphuret, the carbonate, and other profitable ores of copper; in the working of which the ordinary advantages of mining would be greatly enhanced by occasional masses, and veins of native metal. This deduction is rendered probable by the general appearance of the country, and the concurrent discoveries of travellers,—by the green coloured waters which issue in several places from the earth,—by the bodies of native copper found,—by the cupreous tinge which is presented in the crevices of rocks and loose stones,—by the geological character of the country, and by other analogous considerations."

The statement which is made by Mr. Schoolcraft that has particularly induced me to notice his paper is contained in the following paragraph:

"The discovery of masses of native copper is generally considered indicative of the existence of mines in the neighbourhood. The practised miner looks upon them as signs which point to larger bodies of the same metal in the earth, and is often determined, by discoveries of this nature, in the choice of the spot for commencing his labours. The predictions drawn from such evidence, are also more sanguine in proportion to the extent of the discovery. It is not, however, an unerring indication, and appears liable to many exceptions. A detached mass of copper is sometimes found at a great distance from any body of the metal, or its ores; and these, on the contrary, often occur in the earth, or imbedded in rock strata, where there has been no external discovery of metallic copper to indicate it."

The opinion here expressed, and which is given as that of practised miners, is certainly incorrect; it is one indeed which has been often repeated by the writers of books, but which I will venture to add does not rest on the testimony of practical men. Detached, or insulated masses of native copper, or even of the richer kinds of ore, do not of themselves indicate the proximity of valuable mines, and in fact their occurrence is rare even in the most productive districts.

The expenditure of considerable sums in unsuccessful trials, and consequent loss and disappointment, have often been the result of acting upon speculations similar to those quoted by Mr. Schoolcraft, which, though certainly plausible, are not borne out by experience, and which, therefore, it is desirable should be known, are founded only on a popular error.

It is not uncommon to see specimens of ore of great richness exhibited, and to hear it inferred that they are a certain prognostic of valuable mines, whereas experienced miners know that they are to be considered merely as rarities, that they occur frequently in irregular deposits, and seldom in any large quantity. The regular veins from which alone a miner would expect much, are generally enriched in a different manner; and when ore occurs in large masses so as to be worth working, those masses are usually composed of those varieties which are not extraordinarily productive of metal.

Native copper and very rich copper ores do occur indeed in veins, and have been often found in some of our best mines; but they are of themselves not relied on as promising indications, unless accompanied by other favourable symptoms, and must be considered as rare productions, rather than as forming any notable part of the produce of mines.

In judging of the probability of any country being productive of copper, a miner would attend to the appearance or discovery of regular veins,—to their extent,—to the rocks in which they occur,—the substances which they contain,—and to many other things which he would deem more important than any casual specimens of the metal or its ores, and particularly the richer varieties.

He would require at least a combination of some of those indications which experience has shewn to be favourable in order to pronounce that expensive trials were justifiable, or that success was probable; and in forming his judgment, such evidence as is here produced would have but little weight.

With regard to the probability of copper being found in the country described by Mr. Schoolcraft, near Lake Superior, so as to become an object of research, it appears to me that from his report, there is very little to encourage such an expectation. Scattered fragments of native copper are found inclosed in masses of rock, not even *in situ*,—their original situation uncertain,—the rock itself not commonly productive of copper,—the surrounding country alluvial, or composed of red sandstone,—no indication of veins,—and no appearance of substances as are most frequently found accompanying the ores.

The fact is a very curious one, and if, as Mr. Schoolcraft speculates, these masses may have been ejected from volcanoes in the Porcupine Mountains, it would be more desirable to examine those mountains than the district in which they now appear to be placed by some extraordinary chance.

John Taylor.

Article XIV.

Analyses of Books.

1. *Philosophical Transactions for the Year 1821. Part II.*

This part of the Philosophical Transactions contains fifteen papers, the first of which is:

1. *An Account of Experiments to determine the Times of Vibrations of the Pendulums in different Latitudes.* By Capt. Edward Sabine, of the Royal Regiment of Artillery, FRS. and FLS.

For the important communications made by Capt. Sabine to the Royal Society, a Copley medal has been awarded to him. Of this elaborate paper it will be impossible to give even a sketch, for the results of the experiments are contained in upwards of 20 tables, some of which are of great length. The precautions which were taken to ensure accuracy will be best learned from Capt. Sabine's own words. He commences his paper by stating, that "the clocks and pendulums used in these experiments are the property of the Royal Society, and were prepared by their direction under the immediate superintendance of Capt. Kater, who, in a manuscript account presented to the Royal Society, of the instruments furnished to the expeditions on the northern discovery, has described them as follows:

"'The clocks were made by Shelton, and are the same which accompanied Capt. Cook round the world: for each clock, a pendulum was cast in one piece of solid brass: this was furnished with a knife edge of hard steel, perfectly straight, and finished by drawing the edge longitudinally two or three times on a soft hone, so as to take from its sharpness, and thus preclude any alteration from wear; the back of the knife edge bore firmly against a stout cross piece, and the heads of the screws securing it, were sunk below the surface, and concealed by brass pins, to prevent their being removed: the knife edge was carefully adjusted, so as to be at right angles to the direction of the gravity: a very firm support of brass was screwed to the thick plank which forms the back of the clock case; in this were imbedded two pieces of agate, which were ground into portions of hollow cylinders, finished in the places to receive the knife edge of the pendulum: parallel to the agates, a small level was fixed in the direction of the cylinders, by means of which they could be placed truly horizontal: an arc divided into degrees and tenths, but which might be read off by estimation to hundredths, was attached to the back of the clock case at the bottom of the pendulum, to give the arc of vibration.

"'Each clock was furnished with a triangular support of wood contrived by Dr. Wollaston, and so firmly arranged that there appears no reason to apprehend any motion in the point of suspension; and it is sufficiently obvious that no change can take place in the length of the pendulum, but such as may arise from a variation in temperature.'"

Assuming the length of the pendulum vibrating seconds in the latitude of London, viz. 51° 31′ 08·4″ at 39·13929 inches, which has been determined by Capt. Kater, the following table is given by Capt. Sabine, as presenting its length at each of the stations at which the clocks have been set up, deduced from the observations detailed in this paper.

Places of observation.	Latitude.				Length of the pendulum vibrating seconds.
					Inches.
London	51°	31′	08·4″	N	.... 39·13929
Brassa	60	09	42·0		.... 39·16929
Hare Island	70	26	17·0		.... 39·1984
Melville Island ..	74	47	12·4		.... 39·207

Capt. Sabine's paper concludes with deductions as to the figure of the earth; and he gives the following table as containing the diminution of gravity from the pole to the equator, and the resulting ellipticity of the earth deduced from the preceding observations; and the method followed in obtaining these deductions, Capt. Sabine states to be the same as that described by Capt. Kater in the Philosophical Transactions for 1819, p. 420, 421.

	Diminution of gravity.		Ellipticity.
London and Brassa...........	·0055066	....	$\frac{1}{314\cdot3}$
London and Hare Island	·0055082	....	$\frac{1}{314\cdot2}$
Brassa and Hare Island.	·0055139	....	$\frac{1}{313\cdot6}$
London and Melville Island. ..	·0055258	....	$\frac{1}{314\cdot6}$

II. *Some Observations and Experiments on the Papyri found in the Ruins of Herculaneum.* By Sir Humphry Davy, Bart. PRS.

This paper contains, first, a detail of the author's early experiments in England on fragments of papyri; secondly, a description of the rolls in the museum at Naples, and of some analytical experiments made upon them; thirdly, a detail of the various chemical processes carried on in the museum at Naples on the MSS. and of the reasons which induced Sir Humphry to renounce his undertaking before it was completed; and lastly, some general observations on the MSS. of the ancients.

As chlorine and iodine have no action upon pure carbonaceous matter, and a strong attraction for hydrogen, it was conceived that they might be employed to destroy the matter which occasions the adhesion of the leaves, without injuring the carbon of which the ink is composed.

A fragment of a brown MS. the layers of which were strongly adherent, was immediately acted upon by being placed in an atmosphere of chlorine, and the letters became much more distinct; the vapour of iodine had a sensible, but less distinct action.

In using chlorine it was found necessary to employ only a small quantity of it, and when the temperature was properly regulated, the muriatic acid vapour formed assisted the separation of the leaves.

The number of MSS. found at Herculaneum was stated to Sir H. Davy to be originally 1696; of these about one-fourth had been operated on or presented to foreign governments; and on inspecting the state of those which remained, it did not appear that more than from 80 to 120 offered proper subjects for experiments; and this estimate afterwards appeared too high.

Sir Humphry remarks that the persons to whom the care of the MSS. is confided, or who have worked upon them, have always attributed the appearances which they possess to the action of fire, more or less intense; but he is of opinion that the operation of fire is not at all necessary for producing such an imperfect carbonization of vegetable matter as that displayed by the MSS. and he strengthens his opinion by observing, that at Pompeii, which was covered by a shower of ashes that must have been cold, as they fell at a distance of seven or eight miles from the crater of Vesuvius, the wood of the houses is uniformly converted into charcoal; yet the colours on the walls, most of which would have been destroyed or altered by heat, are perfectly fresh, and where papyri have been found, they have appeared in the form of white ashes, as of burnt paper.

Sir H. concludes, that the different states of the MSS. depend upon a gradual process of decomposition, by the action of air and water; the results of the action of heat upon the different specimens of the papyri proved likewise that they had never before been exposed to any considerable degree of temperature.

Only one method, and that a very simple mechanical one, had been used for unrolling the MSS. It consisted in attaching thin animal membrane by a solution of glue to the back of the MSS. and carefully elevating the layers by silk threads when the glue is dry. This, however, was found to be attended with some inconvenience, which Sir H. Davy proposed to obviate by mixing the glue with sufficient alcohol to gelatinize it: in this mode, the alcohol, from its greater lightness, penetrated further into the papyrus, but produced its greatest effect immediately on the first layers. Ether was also tried by Sir H. Davy, and found to be efficacious; it was applied by a camel's hair brush, with precautions, for which we must refer our readers to the paper, as well as for other experiments.

For some time, Sir H. Davy states, that he was at liberty to choose and operate upon specimens; but even after the efficacy and use of the new processes were fully allowed by the unrollers of the Museum, such obstacles were thrown in the way as to induce Sir H. Davy to desist from all further operations.

This paper is accompanied with several engravings of copies of a few of the fragments, selected from nearly 100, for the purpose of showing their nature.

(To be continued.)

2. *An Account of the Interior of Ceylon and of its Inhabitants, with Travels in that Island.* By John Davy, MD. FRS.

Dr. Davy informs us in the preface, that his work is formed from original materials collected in Ceylon during a residence on that station on the medical staff of the army from Aug. 1816, to Feb. 1820.

The substance of the three first chapters is on the physical state of the island in general, and on some particular branches of natural history; and it is to these chapters that our attention will be particularly directed, though much curious matter is contained in other parts of the book which has been collected with a judgment and perseverance that justify the general and high estimation in which its author's abilities are held.

Dr. Davy remarks that the name of Ceylon, familiar to us, but unknown to the languages of the east, is derived probably from Sinhala, the ancient appellation, for which Lakka, and in Pali, Lanka, is now substituted by the natives and commonly used. The island is in the tropic of Cancer, situated nearly between the parallel of 6° and 10° N. latitude, and between 80° and 82° E. longitude.

We shall now proceed to make extracts from the observations contained upon the subjects most interesting to our scientific readers under various heads.

Geographical Notices of the Interior.—The country is low, and almost flat, with the exception chiefly of the southern extremity; Adam's Peak, the Samenella of the Singalese, the most lofty mountain of Ceylon, is about 6,152 feet perpendicular height, and Namana Cooli Kandy, which there is reason to infer is the next loftiest, is about 5,548 feet high.

The character of the interior, in relation to surface, greatly varies. No where is the distinction of high and low land more obvious. With tolerable precision it may be divided into flat country, hilly, and mountainous. The mountainous division is skirted by the hilly, and the latter is generally bounded by flat country. Dividing the island into two equal parts by an imaginary line across, from west to east, the mountainous regions will occupy the middle of the southern half. The centre of this region is about 7° N. and 80° 46′ E. Its great length is about

67 miles, and its greatest width about 53. It is not easy to describe with accuracy the boundaries and extent of the hilly division. The features of each of the three divisions of the interior are necessarily peculiar: grandeur is the characteristic of the mountainous, beauty of the hilly, and sameness of the lowland country, which a covering of luxuriant vegetation, with few exceptions, spread over the whole, does not tend to diminish. In few countries do mountains exhibit greater variety of forms and directions. They most frequently occur connected in chains, and terminating in rounded or peaked summits. I do not recollect a single instance of a solitary insulated mountain. Their sides are always steep, and occasionally precipitous and rocky. In some parts, the chains of mountain observe a parallelism in their course; in other parts, even neighbouring mountains do not correspond with any regularity in their direction.

By some inquirers it is supposed that a correspondence may be traced between the proportional heights of the mountains, and the depths of the adjoining valleys. As a general rule, such a supposition is not applicable to Ceylon. The curious circumstance of there being no lakes, not even a single stagnant pool among the mountains, is alone almost sufficient to show the fallacy of the preceding conclusion.

In the highlands of Scotland, where the loftiest mountain is 2000 feet lower than Adam's Peak, there are many lakes exceeding in depth 600 feet, and it is hardly credible to suppose that lakes of proportional depth ever existed in Ceylon that have since been filled up by the detritus of rocks, little, if at all, more liable to decay and be disintegrated than the rocks of the mountains of Scotland.

Geology and Mineralogy of Ceylon.—In Ceylon, nothing is to be observed of that order and succession of rocks that occur in Saxony and in England, and in many other parts of Europe. Uniformity of formation is the most remarkable feature in the geological character of the island. As far as my information extends, the whole of Ceylon with very few exceptions consists of primitive rock. Another remarkable geological circumstance is, that though the varieties of primitive rock are extremely numerous, and indeed almost infinite, the species are very few, and seldom well defined. The most prevailing species is granite, or gneiss; the more limited are quartz rock, hornblende rock, and dolomite rock, and a few others, which may be considered, perhaps, with advantage under the head of imbedded minerals.

The varieties of granite and gneiss are innumerable, passing from one to another, and occasionally changing their character altogether, and assuming appearances, for which, in small masses, it would be extremely difficult to find appropriate names. Regular granite is not of very common occurrence. One of the best instances I know of it, is in the neighbourhood of Point de Galle, where it is of a grey colour, and fine-grained. Graphic granite is still rarer. The only good example of it with which I am acquainted is at Trincomalee, where it occurs of a beautiful quality, on the sea shore, about half a mile beyond Chapel Point, imbedded in a granitic rock. The quartz, in this instance, is black or grey rock crystal, and the felspar highly crystalline, and of a bright flesh colour. The quartz envelopes the felspar in very thin hexagonal or triagonal cases, so that nothing can be more different in appearance than the longitudinal and transverse fracture of the rock. Neither is sienite common. It occurs, rather forming a part of rocks of a different kind, than in great mountain masses. Well formed gneiss is more abundant than granite. Its peculiar structure may be seen in many places, but no where more beautiful than at Amanapoora, in the Kandyan provinces, where it consists of white felspar and quartz in a finely crystalline state, with layers of black mica, containing disseminated through it numerous crystals of a light coloured garnet. The more limited varieties of primitive rock, as quartz, hornblende, and dolomite rock, seldom occur in the form of mountain masses.

Quartz in large veins and imbedded masses is abundant in the granitic rocks. It is in general milk-white, translucent, full of rents, and so very friable as to remind one of unannealed glass.

Pure hornblende and primitive greenstone are far from uncommon. They constitute no entire mountain or hill that I am aware of, but they form a part of many, particularly of Adam's Peak, and of the hills and mountains adjoining Kandy.

The varieties of dolomite are almost as numerous as those of granite. When purest, it is snow-white, generally crystalline, often highly crystalline, composed of rhombs that are easily separated by a smart blow, but rarely finely granular. I found a specimen of the highly crystalline kind, of specific gravity 1·98, composed of

Carbonate of magnesia	56·0
Carbonate of lime	36·9
Alumina	4·1
Silica	1·0
Water	2·0
	100·0

A very fine granular kind, of a beautiful whiteness, well adapted for statuary purposes, is found in the neighbourhood of Fort Macdonald. A specimen of it that I tried was of specific gravity 2·74, and contained only a very small proportion of carbonate of magnesia. The varieties of most importance are mixtures of dolomite with felspar and mica, and even quartz. It is in rocks of this kind that the nitre caves of the interior are found.

In external character and general structure, the varieties of primitive rock exhibit fewer marked differences than might be expected, *à priori*.

The recent formation is highly deserving of investigation, both as a partial exception to the comprehensive idea, that the whole island is composed of primitive rock, and on account of its own interesting nature. The rock that occurs in this formation, is of two kinds, limestone and sandstone; both of these may become very useful. Very good lime may be made of the former, and serviceable millstones may, perhaps, be made of the latter, if it can be found, as is very probable, of a coarse quality.

We beg to refer our readers to the original for a more extended account of the geology of this island, aware that our limits will not allow us to make more copious extracts; and proceed to consider its

Mineralogical Productions.—The mineralogy of Ceylon is, in certain respects, singular and curious. The island is remarkable for its richness in gems, and its poverty in the useful metals. It is remarkable too for the number of rare minerals that it affords, and for the small variety of the ordinary species: thus, in its mineralogical character, quite oriental, better fitted for show than utility, for pomp than profit.

Its mineral productions may be considered under two heads; namely, those that belong to granitic rock, and those that belong to dolomite rock.

The only metallic ores hitherto found in Ceylon are of iron and manganese. Iron, in different forms, is pretty generally diffused, and tolerably abundant. I have met with the following species: Iron pyrites, magnetic iron ore, specular iron ore, red hematite, bog iron ore, and earthy blue phosphate of iron. Red hematite and bog iron ore are more common than the other species. It is from these ores that the natives extract the metals. Iron pyrites is rare: it is to be met with at Ratnapoora, in Saffragam, disseminated through a grey felspar rock, and in veins of quartz, at Mount Lavinia, on the sea shore. Magnetic iron ore I have found in masses, imbedded in gneiss, in the neighbourhood of Kandy, and in a granitic rock at Katabowa, in Welassey, and disseminated through a similar rock at Trincomalee. The earthy blue phosphate of iron has been procured from a marshy ground in the neighbourhood of Colombo, and from a bed of bog-iron ore, near Atgalle, not far from Kandy. It is said to be used by the natives as a pigment. It is worthy of notice, that no great bed, and that no vein of iron ore, has been found in Ceylon. Only one ore of manganese, viz. grey manganese, or the black oxide, is yet known in Ceylon. I first discovered it, about two years ago, in several parts of Saffragam and of Upper Ouva.

From the nature of the rock, it might be expected that other metals would be found in Ceylon. It may be remarked, it is not for want of search they have not been discovered. "Wherever," says our traveller, "I have been amongst the mountains, I have sought more particularly for tin and copper, but in vain, having never observed the slightest traces of either, or of lead. Most of the gems for which Ceylon is celebrated, occur, I believe, in granitic rock.

Belonging to the quartz family may be enumerated quartz, iron-flint, chalcedony, and hyalite. Ceylon affords all the varieties of quartz, as rock-crystal, amethyst-rose-quartz, cat's-eye, and prase. Rock crystal occurs in abundance, both massive and crystallized, of various colours, good quality, and in large masses. The natives use it instead of glasses for the lenses of spectacles; they employ it too for ornamental purposes, and in statuary. Amethyst, also, is pretty abundant. Very beautiful specimens of this mineral are found in the alluvion, derived from the decomposition of gneiss and granitic rocks, in Saffragam and the Seven Korles. I have seen a large crystal of it, lately found near Ruanwelle, containing apparently two distinct drops of water. Rose quartz, which is pretty common, is often found in the same place as amethyst. Ceylon produces the finest cat's-eyes in the world, indeed the only kind that is highly esteemed, and that brings a high price. Prase is a variety of quartz that is of rare occurrence in the island. The second species, iron flint, is not uncommon in the Kandyan country. The third species, chalcedony, there is strong reason to suppose, exists in the mountains of the interior. The fourth species, hyalite, is extremely rare; I have met with it only in a nitre cave in Doombera, partially encrusting a granitic rock.

Belonging to the schorl family, I am acquainted with two species only that undoubtedly occur in Ceylon, which are topaz and schorl. The topaz generally passes under the name of the "white or water sapphire." It is generally white, or bluish or yellowish white; it is commonly much water-worn, and perfect crystals of it are very rare. Schorl I have not found in that abundance I expected: common schorl indeed is not uncommon. Tourmaline is rare; the few specimens I have seen of it of the green, honey-yellow, and red varieties, were of bad quality, and I could not ascertain their locality. It is the opinion of some writers, that both the emerald and beryl are found in Ceylon. The former certainly is not found, and it is even doubtful if the latter is. Of the garnet family, three species occur in gneiss or granitic rock, viz. the garnet, pyrope, and cinnamon stone.

The Zircon family is richer in Ceylon than in any other part of the world. Besides the two well established species zircon and hyacinth, I have met with a third, massive, opaque, and uncrystallized, and of a dark-brown colour; I have specimens of it weighing two or three ounces from Suffragam.

For the ruby family Ceylon has long been celebrated. Four species of it, viz. spinell, sapphire, corundum, and chrysoberyll, occur, I believe, in gneiss, or granitic rock. Spinell is comparatively rare; sapphire is much more common.

The purple variety or the oriental amethyst is rare; a green variety is still rarer. The black sapphire too is rare. Corundum is less frequently met with than the sapphire. I know of one place where it abounds, and I am not aware that it has been found any where else in the island. Corundum is the only species of this family not considered a gem, and the only one that is applied to any purpose of utility. In the state of fine powder it is largely employed by the lapidary in cutting and polishing stones, and by the armourers in polishing arms.

Of the felspar family, it is highly probable, that several species exist in the island. I have met with all the subspecies of felspar.

Of the hornblende family I am acquainted with two species only that occur in Ceylon, viz. common hornblende, and glassy tremolite.

Pitchstone is the only mineral of the family of this name I have ever found in Ceylon.

Mica, as a constituent part of granite and gneiss, is abundant. Common chlorite is to be met with occasionally. Green earth is more rare: this mineral is of an unusually light colour, varying from green to light apple-green.

Magnesian minerals are far from abundant in Ceylon. The only minerals of this kind that I met with were dolomite, carbonate of magnesia, and talc. The very rare mineral, native carbonate of magnesia, I discovered in a nitre cave in the valley of Maturatta, accompanied with dolomite, and encrusting and included in gneiss. The best specimens of it were of a pure snow-white, earthy texture, rather harsh to the feel, destitute of smell when breathed on, and not adhering to the moist tongue. It varied in specific gravity from 2·32 to 2·70, according to its compactness. One specimen of it that I examined was composed of

Carbonate of magnesia	86
Water	5
Silica, with slight traces of carbonate of lime.	9
	100

Calc spar, anhydrous gypsum, and calc sinter, are the only pure calcareous minerals that I have observed in Ceylon.

Belonging to the inflammable class of minerals I know of only two that occur in Ceylon, viz. graphite and sulphur, the latter is extremely rare in Ceylon; indeed its occurrence is not yet demonstrated in a manner perfectly satisfactory.

The mode in which gems are sought for is so simple that it hardly deserves the name of an art. It is only in alluvial ground, it has been remarked, that these scarce and beautiful minerals have yet been discovered in Ceylon. Where there is a probability of finding them, pits are sunk from 3 to 20 feet deep; the coarse sand and gravel through which they are generally disseminated is collected and carried in baskets to an adjoining stream, where it is well washed; the lighter particles are got rid of by a rotary motion given to the basket in the operation; and the residue, still wet, is transferred to shallow baskets for careful examination.

Ores of Iron and Manganese, it has been observed, are the only ores that have yet been discovered in Ceylon. With the mode of reducing the former, and of working the iron which they extract, the natives are well acquainted. Their process of smelting iron, like most of their other processes, is remarkable for its simplicity. The most complete Singalese smelting-house that I ever visited consisted of two small furnaces under a thatched shed.

Salts.—The saline productions of Ceylon are far from numerous. The only salts, the existence of which I have ascertained in a satisfactory manner, are the following; viz. nitre, nitrate of lime, sulphate of magnesia, alum, and common salt. These salts, with the exception of common salt, have been found hitherto in the interior only, and in certain caves, where, not being liable to be washed away by the heavy tropical rains, they admit of being detected.

Nitre and nitrate of lime are of frequent occurrence. Judging from four nitre caves that I have visited, and from the specimens of rocks of several more that I have examined, I believe that they are all very similar; and that the rock in which they occur, in every instance, contains at least felspar and carbonate of lime, from the decomposition of the former of which, the alkaline base of the salt is generally derived, and by the peculiar influence of the latter (yet not at all understood) on the oxygen and azote of the atmosphere, the acid principle is generated.

Nitre Cave of Memoora.—The first view of the place was exceedingly striking. A large cave appeared in a perpendicular face of rock about 300 feet high crowned with forest, at the base of which was a stage or platform of rubbish, that seemed in danger of sliding into a deep wooded valley, closed in by mountains of considerable elevation, and remarkable boldness. The cave was 200 feet deep, and at its mouth, which was nearly semicircular, about 80 feet high, and 100 wide. Its floor was rocky and steep, rapidly ascending inward, and its extremity was narrow and dark. To facilitate the ascent, ladders were placed in the most difficult situations. The nature of the rock of which the walls of the cave are formed, has already been described. The workmen whom I found at their labours, 16 in number, were the rudest set of artificers I ever witnessed; their bodies almost naked were soiled with dirt, and their bushy beards and hair were matted and powdered with brown dust. When I arrived, they were occupied, not in the cave, but on the platform before it, attending to the operations that were then going on in the open air, of filtration, evaporation, and crystallization. The apparatus employed was curious from its simplicity and rudeness. A small stream of water was led from a distance to the place by a pipe of bamboos; the filters were of matting, in the shape of square boxes, supported by sticks; and the evaporating vessels, and indeed all the vessels used, were the common chatties of the country, of which a great many were assembled of various sizes. The cave may be considered partly natural, and partly artificial. I was informed that during the last 50 years, for six months in the dry season, it has been annually worked, and that each man employed was required to furnish a load of nitre, which is about 60 pounds, to the royal stores."

Saltpetre.—The preparing of saltpetre, and the manufacture of gunpowder, are arts which the Singalese, for many years, have constantly practised. The process of preparing the salt, in different parts of the country, was very similar. When the salt occurred impregnating the surface of the rock, as in the cave near Memoora, the surface was chipped off with small strong axes, and the chippings by pounding were reduced to a state of powder. This powder, or the loose fine earth, which, in most of the caves, contained the saline impregnation, was well mixed with an equal quantity of wood-ash. The mixture was thrown on a filter formed of matting, and washed with cold water. The washings of the earth were collected in an earthen vessel, and evaporated at a boiling temperature till concentrated to that degree that a drop let fall on a leaf became a soft solid. The concentrated solution was set aside, and when it had crystallized the whole was put on a filter of mat. The mother-lye that passed through, still rich in saltpetre, was added to a fresh weak solution to be evaporated again; and the crystals, after having been examined, and freed from any other crystals of a different form, were either immediately dried, or, if not sufficiently pure, redissolved and crystallized afresh. The operations just described were generally carried on at the nitre caves. In the province of the Seven Korles, besides extracting the salt at the caves, the workmen brought a quantity of the earth to their houses, where, keeping it under a shed protected from the wind and rain, without any addition excepting a little wood-ash, they obtain from it every third year a fresh quantity of salt.

Gunpowder.—In their mode of manufacturing gunpowder, which is very generally understood, there is not the least refinement. To proportion the constituent parts, scales are used, but not weights. The proportions commonly employed are five parts of saltpetre, and one of each of the other ingredients of sulphur and charcoal. The charcoal preferred is made of the wood of the parwatta tree. The ingredients moistened with very weak lime water, and a little of the acrid juice of the wild yam, are ground together between two flat stones, or pounded in a rice mortar. After the grinding or pounding is completed, the moist mass is exposed to the sunshine to dry. Nothing further is done to it; no attempt is ever made to granulate it, and it is used in the state of a very coarse powder, or impalpable dust. Considering the rudeness of the method, the gunpowder is better than could be expected. Some specimens of it that have been examined have inflamed readily, exploded very strongly, and have left little residue.

Common salt forms in great quantities in certain lakes on the sea-shore, but of rare occurrence indeed in the interior, except in very minute quantity. Dr. Davy has given a detailed account of the manner in which salt is procured, and is decidedly of opinion, that the sea is the source from which the salt is derived, and that evaporation is the cause of its production or forming.

Dr. Davy observes, that the importance of the subject is greater than it may appear to a casual reader, the monopoly of salt of the Megam-pattoo yielding government a revenue of at least 10,000*l.* a year, and the whole island being almost entirely dependent on this district for the supply of this necessary of life. He adds: Were the salt lakes scientifically managed, they might be made to yield not only any quantity of common salt to supply all India, but almost any quantity of magnesia might be extracted from the residual brine.

Jewellery.—The Singalese work in gold and silver with considerable dexterity and taste; and, with means that appear very inadequate, execute articles of jewellery that would be admired certainly in this country, and not very easily imitated. The best artist requires only the following apparatus and tools:—a low earthen pot full of chaff or saw-dust, on which he makes a little charcoal fire; a small bamboo blowpipe, about six inches long, with which he excites the fire; a short earthen tube or nozle, the extremity of which is placed at the bottom of the fire, and through which the artist directs the blast of the blowpipe; two or three small crucibles made of the fine clay of ant-hills; a pair of tongs; an anvil; two or three small hammers; a file; and to conclude the list, a few small bars of iron and brass, about two inches long, differently pointed for different kinds of work. It is astonishing what an intense little fire, more than sufficiently strong to melt silver and gold, can be kindled in a few minutes in the way just described. Such a simple portable forge deserves to be better known: it is, perhaps, even deserving the attention of the scientific experimenter, and may be useful to him when he wishes to excite a small fire, larger than can be produced by the common blow-pipe, and he has not a forge at command. The success of the little Singalese forge depends a good deal on the bed of the fire being composed of a combustible material, and a very bad conductor of heat.

It would be tedious to enumerate the variety of work a native blacksmith is equal to; locks, and even gun-locks and gun-barrels, do not exceed his abilities. The workmanship is indeed coarse, and not to be praised, but still they answer pretty well the purpose for which they were intended, and give satisfaction to those unacquainted with better. The smiths use a composition as a hone in sharpening knives, and cutting-instruments, that is worth noticing. It is made of the capitia resin and of corundum. The corundum, in a state of impalpable powder, is mixed with the resin rendered liquid by heat, and well incorporated. The mixture is poured into a wooden mould, and its surface levelled and smoothed while it is hot; for when cold, it is extremely hard. It is much valued by the natives, and preferred by them to the best of our hones.

In concluding our extracts from this volume, it would be injustice to the author not to remark, that it is written in a plain and clear style, and embraces many topics of general information. It is embellished with maps, numerous engravings, and cuts, which serve well to illustrate the entertaining particulars contained in the text.

ARTICLE XV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Nov. 8.—The first meeting of the Society took place this day, when the Croonian lecture, on the "Adjustment of the Eye," by Sir E. Home, Bart. was commenced.

Nov. 15.—The above lecture was continued and concluded.

Nov. 22.—A paper was read, entitled, "Experiments to determine the Amount of the Dip of the Magnetic Needle in London, in August, 1821." By Capt. E. Sabine.

Nov. 30.—This being St. Andrew's Day, the Society held their annual meeting, when the following gentlemen were elected officers for the year ensuing:

President.—Sir Humphry Davy, Bart.

Treasurer.—Davies Gilbert, Esq. MP.

Secretaries.—William Thomas Brande, Esq. and Taylor Combe, Esq.

Council.—Bishop of Carlisle; C. Hatchett, Esq.; J. F. W. Herschel, Esq.; Sir E. Home, Bart.; John Pond, Esq.; W. H. Wollaston, MD.; Thomas Young, MD.; Earl of Aberdeen; Matthew Baillie, MD.; John Barrow, Esq.; B. C. Brodie, Esq.; William Hamilton, Esq.; James Ivory, Esq.; Marquis of Lansdowne; Alexander Marcet, MD.; Thomas Murdoch, Esq.; Sir Robert Seppings, Knt.

Sir H. Davy, the President, delivered a discourse on presenting two Copley medals, to J. W. Herschel, Esq. and Capt. Edward Sabine, RA. In the limits to which we are necessarily restricted, it is quite impossible to do justice to this discourse, or to convey to the reader an adequate idea of the profound attention and respect with which it was received by the Society; but we shall attempt to sketch an outline of some of the more striking and interesting parts of it.

The President began by observing, that the progress of discovery is always an agreeable subject of contemplation, which is increased when it arises from the talents of our own countrymen, especially when connected with the power of distinguishing them by a lasting token of respect. The President then stated his conviction that the Society would participate in the satisfaction he felt in the decision of the Council, in awarding Copley medals to the gentlemen already named.

Alluding to the labours of Mr. Herschel, Sir Humphry Davy observed, that no branch of science is so calculated to excite admiration as the sublime or transcendental geometry, as showing the wonderful powers of the human mind, and demonstrating the beauty and wisdom of the system of the universe. It must be gratifying to the Society, he observed, to see Mr. Herschel who, at an early period of life, had gained academical honours of the highest kind, successfully continuing his pursuit of that kind of knowledge by which, from the labours of Newton, the Royal Society had acquired so much glory. Sir Humphry then mentioned that Mr. Herschel has contributed four papers to the Transactions of the Society on pure mathematical subjects; the merits of these, he observed, could only be appreciated by deeply studying them. Mr. Herschel, the President continued, had not confined himself to the invention and development of formulæ, but had made important applications of them, and that although the higher mathematics strengthen the reasoning faculties, and afford intellectual pleasure, yet their grandest end and use are in solving the physical phenomena of the universe, and modifying the properties of matter. Sir H. then alluded to two other papers of Mr. Herschel, in the Transactions of the Society, on Physico-Mathematical subjects, connected with optical phenomena. In the first of these papers, on polarized light, the author was stated to have added to the subject, by some novel investigations, and had reduced the explanation of the phenomena to one general fact. In this paper Mr. Herschel had extended or modified the views of others, but the second on the aberrations of compound lenses and object glasses was more original, and was, as the President observed, on a subject highly important to practical optics, by enabling artists to substitute mathematical rules for empirical methods in working their glasses; thus adding, said the President, "to the immense obligations owing to the name of Herschell in every thing connected with the progress of modern astronomy, and the knowledge of celestial phenomena."

On presenting the medal to Mr. Herschel, the President desired him to receive it as a mark of the respect and admiration of those talents which he had applied with so much zeal and success; and to preserve it as a pledge of future exertions in the cause of science and of the Royal Society; and he assured him that he could communicate his labours to no public body by whom they would be better received, or through whose records they would be better known to the philosophical world.

"Mr Herschel," the President observed, "was in the prime of life, in the beginning of his career, and had powers and acquirements capable of illustrating and extending every branch of physical inquiry; and in the field of science, there were spots not yet investigated, or not yet cultivated. Where the laws of sensible become connected with those of insensible motions, the mechanical with the chemical phenomena, he observed that little was known; and that in electricity, magnetism, heat, the relations of the crystallized forms to the weights of the elements of bodies, there were a number of curious and important objects of research.

"May you continue," said the President, in concluding his address to Mr. Herschel, "to devote yourself to philosophical pursuits, and to exalt your reputation, already high, 'Virtutem extendere factis,' and these pursuits you will find not only glorious, but dignified, useful, and gratifying, in every period of life: this indeed," continued the President, "you must know best in the example of your illustrious father, who, full of years and of honours, must view your exertions with infinite pleasure, and who in the hopes that his own imperishable name will be permanently connected with yours in the annals of science, must look forward to a double immortality."

In speaking of the researches of Capt. Sabine, the President observed, that the expeditions to the Arctic regions, which had been planned with much liberality and sagacity, had awakened strong interest in the public mind, and he observed that it would be unnecessary to point out the particular merits of those bold and enterprising persons who had devoted themselves to the cause of science and their country.

As, however, Capt. Sabine had been appointed Astronomer and Philosophical Observer to the two first of these expeditions, in consequence of the recommendation of the Council of the Royal Society, they had thought it right to express their sense of his merits by awarding him a Copley medal.

The President observed, that Capt. Sabine had shown great industry and perseverance in making experiments under peculiarly difficult circumstances, and had accumulated an immense number of observations in astronomy and meteorology, and in the phenomena of magnetism and gravitation; and the principal of the experiments were conducted on the ice of the polar sea, where the vessel was for several months frozen up. During a considerable part of the time he was in darkness, or only guided by a very doubtful twilight; and such was the intensity of the cold, that the artificial horizon of mercury became frozen during an observation, and yet, continued the President, Capt. Sabine's experiments seem to have been conducted with as much care and precision as if he had possessed the conveniences of an Observatory, and the advantages of the happiest climate.

The President stated, that three papers by Capt. Sabine had been published in the Philosophical Transactions, the two first relating to magnetic phenomena, and the last containing an account of experiments on the acceleration of the vibrations of the pendulum in different latitudes.

The President then entered into some details on the subject of Capt. Sabine's last paper, and stated the results of his experiments, and he observed, that he was now gone to complete his investigation even to the line; "having braved the long night, and almost perpetual winter, of the polar regions, he is gone with the laudable object to expose himself to the burning sun and constant summer of the equator."

Capt. Sabine, not being present for the reason already stated, the President delivered the medal to his brother, requesting that in apprising him of what has taken place, he would state to him the deep sense entertained of his merits. His knowledge of the expression of the opinion of the Royal Society may, perhaps, said the President, animate him during the difficult enterprize he has undertaken, as he had already shown how highly he values the praise of the Royal Society, which, with the good opinion of his countrymen, had been hitherto the only reward of his labours. "Assure him," said the President, "how strongly we feel his disinterestedness and genuine love of science, and that our ardent wishes are expressed for his safe return, and for the successful accomplishment of all the objects of his voyage, which will ensure to him additional claims upon the gratitude of all lovers of science."

Dec. 23.—On some Alvine Concretions found in the Colon of a young Man, in Lancashire after Death, by J. G. Children, Esq. FRS. &c.

It appears from the statements contained in this communication, that the young man, whose case it relates, had eaten at various times a large quantity of plums, and generally swallowed the stones. After some time, a hard circumscribed tumour was discovered on one side of the abdomen, which was distinctly felt to be an alvine concretion. The usual remedies were applied in vain for removing it, and after having been attended for about three months by a medical man he died.

On opening the body, three closely compacted concretions were found rather high upon the left side, and a fourth considerably lower. This last was sawn asunder by the medical gentleman who opened the body, and was found to contain a plum stone in the centre.

The total weight of the concretions in the state in which Mr. Children received them was about 4¾ ounces; the largest weighing 1036 grains, and the smallest about 511 grains. The specific gravity of the largest was 1·875.

100 parts yielded, by analysis, animal matter, chiefly gelatine	25·2
Resin	3·9
Ammoniaco-magnesian phosphate	5·16
Phosphate of lime	45·34
Vegetable fibre	20·3
	99·9

The vegetable fibre appeared to be derived from the oatmeal which forms a considerable proportion of the food of the labouring class in Lancashire.

On the same day, a paper was read, by Dr. Wollaston, on the Adjustment of Chromatic Object Glasses.

At the same meeting, a paper was read, by Sir Everard Home, on a new Species of Rhinoceros found in the Interior of Africa.

Article XVI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Comet.*

The *Sydney, or New South Wales Gazette* of April 7, mentions a beautiful comet at that period visible in the hemisphere. It formed a triangle to the south-west, with the west shoulder of Orion and Aldebaran.

II. *Plymouth Breakwater.*

M. Dupin furnishes a very curious estimate of the number of persons employed on this chef-d'œuvre of naval architecture, and the quantity of stone sunk by each individual. On contrasting this with the parallel works at Cherburgh, it appears that three persons at Plymouth were enabled to accomplish as much as four at the latter place, in the same period of time.

III. *Ventilation of Rooms.*

Mr. Perkins has suggested an improved mode of ventilating and warming rooms. It consists in introducing a column of cold air immediately at the back of the stove, and by this means, a large portion of the heat usually wasted or misapplied, is equally diffused over the room. The greater the quantity of air which is made to strike against and pass by the stove, the greater is the quantity of heat given out by it. It will not, however, work to good advantage when the room is air tight, and to remedy this evil, it is adviseable to pierce an aperture in the ceiling, or by opening a door in an adjoining apartment produce the necessary current.

IV. *Lampyris Noctiluca and Splendidula.*

In a curious paper on the phosphorescence of these animals, M. Macaire has drawn the following conclusions, which he gives as the result of a variety of observations:—1. A certain degree of heat is necessary to their voluntary phosphorescence. 2. Their phosphorescence is excited by a degree of heat superior to the first, and is irrecoverably destroyed by a higher temperature. 3. All bodies capable of coagulating albumen take away from phosphorising matter its power of phosphorescence. 4. The phosphorescence cannot take place but in a gas which contains no oxygen. 5. It is excited by the galvanic pile, but no effect is produced upon it by common electricity; and 6. The phosphorescent matter is composed principally of albumen.—(Bibliotheque Universelle.)

V. *New Analyses of Meteoric Iron.*

Dr. John, of Berlin, has lately submitted to analysis, specimens of the meteoric iron, which is disseminated in the aërolites of Chatonnay, of l'Aigle, and of Sienna; the following are the results of his experiments.—

Iron of the aërolite,

	Of Chatonnay.	Of l'Aigle.	Of Sienna.
Iron,	92·72	92·72	92·72
Nickel,	5·50	5·50	5·10
Sulphur,	1·00	Minute quantities which were not weighed.	
Cobalt,	0·78		
Chrome a trace.			
	100·00		

Dr. John states, that by comparing these results with those of the analyses of the great masses of malleable iron, to which a meteoric origin is usually attributed, it is found,

1. That the iron of aërolites, and the malleable iron in large masses contain the same substances, viz. iron, nickel, cobalt, chrome, and perhaps a trace of manganese, which Dr. J. discovered in the iron of Ellbogen.

2. It appears that the iron of aërolites does not contain quite so much nickel as the great malleable masses.

3. The iron of aërolites evidently contains sulphur; but as it is at the same time very malleable, it is probable that the sulphur is not combined with the whole of the iron, but only with a small portion; and arising from the magnetic pyrites disseminated through the whole mass. The great masses of iron prove this assertion, for when they are very malleable and ductile, as the iron of Pallas, that of Humboldt, that from Ellbogen, &c. they do not contain any trace of sulphur. It has been said that the iron discovered in Siberia by Pallas, does contain a portion of this substance; but Dr. John could not discover any in it.—(Ann. de Chim.)

ARTICLE XVII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Mr. Crabb, author of "English Synonyms Explained," has in the press "A Universal Technological Dictionary," containing the explanation of all terms of science and art, drawn from the most approved writers ancient and modern. The work will be completed in Two Quarto Volumes, and will be illustrated by numerous plates, diagrams, and cuts. It will be published in Monthly Parts, the first of which will appear on March 1. A Prospectus of the Work is nearly ready.

Illustrations of the History, Manners, and Customs, Arts, Sciences, and Literature, of Japan; selected from Japanese Manuscripts, and printed Works. By M. Titsingh, formerly chief Agent of the Dutch East India Company, at Nangasaki; and accompanied with many coloured Engravings, faithfully copied from Original Japanese Paintings and Designs.

Remarks on Cutaneous Diseases. By Mr. Wilkinson.

The Principles of Medicine, on the Plan of the Baconian Philosophy, Vol. I. On Febrile and Inflammatory Diseases. By Mr. R. D. Hamilton.

JUST PUBLISHED.

An Epitome of Pharmaceutical Chemistry. By Rees Price, MD. 12mo. 3*s*. or on a Chart adapted for framing, 2*s*. 6*d*.

A Natural Arrangement of British Plants according to their Relations to each other, as pointed out by Jussieu, De Candolle, Brown, &c. including those cultivated for Use, with the Characters, Differences, Synonyms, Places of Growth, Time of Flowering, and Sketch of their Uses, with an Introduction to Botany, in which the Terms newly introduced are explained. By Samuel Frederick Gray, Lecturer on Botany, &c. with 21 Plates, in Two very large Volumes, 8vo. 2*l*. 2*s*.

Pathological and Surgical Observations on Diseases of the Joints. By B. C. Brodie, FRS. Assistant Surgeon to St. George's Hospital, and Lecturer on Surgery. 8vo. With Plates. 16*s*.

Generic and Specific Descriptions of the Carnivorous Animals, which are intended to form a *distinct* Treatise on the Order Carnivora, as well as the second Part of the History of Vertebrated Animals. By Edward Griffith. No. I. Price 10*s*. 6*d*. to be completed in Six or Seven Numbers.

*** This Work will be illustrated with coloured impressions of Copper-plate Engravings, all from Original Drawings, many of which are of undescribed species and varieties.

A Letter to Dr. Parry, on the Influence of Artificial Eruptions in certain Diseases, &c. By Edward Jenner, MD. &c. 4to. 5*s*.

A System of Pathological and Operative Surgery, founded on Anatomy, with Engravings. By Robert Allan, FRS. Vol. II. 12*s*. 6*d*.

Miscellaneous Works of the late Robert Willan, MD. Edited by Ashby Smith, MD. 8vo. 12*s*.

Article XVIII.

METEOROLOGICAL TABLE.

1821.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Hygr. at 9 a.m.	
		Max.	Min.	Max.	Min.				
11thMon.									
Nov. 1	S W	30·00	29·98	60	55	—	—		
2	W	29·98	29·90	63	55	—	13		
3	W	29·90	29·42	57	40	—	39		☾
4	W	30·16	29·42	46	28	—	10		
5	W	30·38	30·19	54	28	—			
6	S W	30·38	30·35	41	30	—			
7	E	30·35	30·26	48	39	—			
8	E	30·26	30·22	48	36	57			
9	E	30·22	30·19	46	38	—			○
10	S E	30·19	30·04	58	44	—			
11	S	30·04	29·93	55	45	—	75		
12	W	30·03	29·93	56	35	—	01		
13	E	29·93	29·86	55	40	—	—		
14	S W	29·86	29·82	58	52	—	12		
15	S W	29·82	29·57	61	51	—	02		
16	S W	29·65	29·57	56	50	—	55		☽
17	S W	29·96	29·65	54	47	—	1·07		
18	N	30·07	29·96	50	41	38			
19	S W	30·07	30·00	52	42	—	28		
20	S W	30·00	29·75	52	45	—	07		
21	N W	29·75	29·65	50	38	—	09		
22	S W	29·77	29·60	55	45	—	03		
23	S W	29·90	29·75	54	35	—	15		
24	S W	29·86	29·77	52	41	—	—		●
25	N W	29·77	29·45	55	46	—	25		
26	S W	29·58	29·36	56	42	—	32		
27	N W	29·86	29·58	44	30	—			
28	S W	29·84	29·69	55	43	—	04		
29	S W	29·94	29·84	54	40	—	14		
30	N W	29·97	29·67	55	42	56	16		
		30·38	29·36	63	28	1·51	4·67		

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Eleventh Month.—1, 2. Cloudy. 3. Rainy: very stormy night: the wind blowing quite a gale. 4. Stormy. 5—8. Fine. 6, 7. Lunar halo and corona. 9. Fine: *Cirrus:* lunar halo. 10. Fine: cloudy. 11. Rainy. 12. Fine. 13. Cloudy. 14. Cloudy. 15. Cloudy. 16. Rainy: squally. 17. Cloudy: rainy night. 18. Fine. 19. Rainy morning: fine afternoon. 20. Cloudy. All the marshes in the neighbourhood flooded to a considerable depth from the rains of the last few days. 21. Cloudy. *Cirrocumulus* and *Cirrostratus* in the afternoon. 22. Rainy morning: 23. Cloudy: drizzly. 24. Rainy. 25. Fine: stormy night. 26. Fine morning: rainy afternoon. 27. Fine. 28. Drizzling: very cold wind. 29. Fine. 30. Fine day: stormy night.

⁂ *Daniell's Hygrometer* indicated a depression of 15° at noon on the 7th; on the 8th, 9°; on the 9th, 10°; and on the 10th, 5°. This gradual approach to the point of saturation, during a succession of fine days, followed, as will be seen, by wet weather, deserves notice. Observations with this instrument will be given occasionally in future.

RESULTS.

Winds: N, 1; E, 4; SE, 1; S, 1; SW, 14; W, 5; NW, 4.

Barometer: Mean height	
For the month	29·897 inches.
For the lunar period, ending the 16th	29·906
For 14 days, ending the 5th (moon south)	29·974
For 13 days, ending the 18th (moon north)	30·004
Thermometer: Mean height	
For the month	47·383°
For the lunar period	48·344
For 30 days, the sun in Scorpio	47·833
Evaporation	1·51 in.
Rain	4·67

Laboratory, Stratford, Twelfth Month, 21, 1821. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1822.

Article I.

Analysis of the Variegated Copper Ore, or Buntkupfererz. By R. Phillips, FRSE. FLS. &c.

In looking over the analyses of the various sulphurets of copper and iron, I was struck not only with the different results obtained by analysts of great experience, but also with the difficulty of reconciling any of their statements with the idea that these sulphurets are definite compounds of the ingredients of which they are constituted. That they are of regular composition can, I think, hardly be questioned when specimens from different countries are compared, and especially when it is considered that they are all occasionally met with in the crystalline state; and have generally different primary forms.

For the reasons which I have now stated, I propose to examine the native sulphuret of copper and the sulphurets of copper and iron, and with this intention I shall now state the experiments which I have performed upon that which, from accidental causes, first attracted my notice, namely, the variegated copper ore, or buntkupfererz of the Germans.

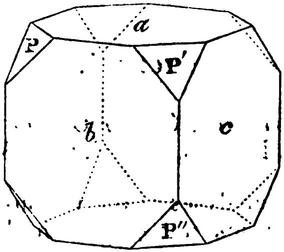
This ore is thus described in Phillips's Mineralogy:

"Its colour seems to consist of an intimate mixture of copper red and tombac brown, with an irridescent tarnish, generally of blue, sometimes yellow. The fracture is imperfectly conchoidal occasionally, more often fine grained and uneven; it is soft, easily frangible, and sectile in a slight degree. Specific gravity 5·033."

On the subject of the crystalline form of this substance, my brother (Mr. W. Phillips) has presented me with the following remarks:

"In some treatises on mineralogy, the buntkupfererz is cited as being found in cubes of which the solid angles are replaced, and in cubes of which the planes are curvilinear. The Abbé Haüy, however, in his Traité, notices it under the name of 'cuivre pyriteux hepatique,' considering it as resulting from copper pyrites, and, as it may be assumed, by some natural transition analogous to the known passage of the red oxide of copper of Chessey into the green carbonate; and he quotes it under the same name as an appendix to copper pyrites in his Tableau Comparatif.

"Being in the possession of a specimen from Cornwall, on which there are many well defined and brilliant crystals, I detached some, and have obtained from one of them, which is in the form of the cube having all the solid angles replaced, the following results by means of the reflective goniometer:



a on *c*	90°	10′
a on *c* return	89	45
a on *b*	90	22
a on *b* return	89	30
c on *b*	90	8
c on *b* return	89	42
P′ on P	109	40
P′ on *a*	125	16
P′ on *c*	125	0
P′ on *b*	125	35

"If evidence were wanting to show that the crystal is in the general form of the cube, it might be added that the average of the six measurements *a* on *b* and *c* and their return planes, is so near 90° as to amount to 89° 56′, and that the planes P′ P″ and their opposed planes, together with the plane *a* and its return plane, may be measured by simply turning the crystal on the axis of the goniometer so as to show each of those planes in succession.

"In attempting to cleave the crystals of this substance, I have not been so thoroughly successful as could be wished. Cleavages are, however, attainable parallel to all the planes P, though not sufficiently brilliant for the use of the reflective goniometer; but determinate enough to satisfy me that the primary crystal is the regular octohedron.

"The primary octohedron, besides the modifying planes by which it so nearly passes into the cube, is also subject to another modification, causing it sometimes to assume a form which might be mistaken for a rhomboid; but its planes are not sufficiently defined on any of the crystals in my possession to allow of measurement or determination.

"The preceding measurements and remarks it is presumed will suffice to show that this substance is not derived from copper pyrites, and that its crystalline forms are not in any degree allied to those of that substance, which does not occur in the form of the regular tetrahedron or octohedron.

"It may not be amiss to add, that the buntkupfererz is sometimes found in the mines of Cornwall *apparently* in the form of the six-sided prism, frequently tabular, or in crystals which approach in form to a double six-sided pyramid with triangular planes, and which are allied to the six-sided prism; the use of the knife, however, will always, as far as my observation goes, evince that it is merely a coating of the buntkupfererz, on crystals of the vitreous copper."

Klaproth has given two analyses of this ore, one specimen being from Hitterdahl, in Norway, and the other from Rudelstadt, in Silesia: the results are thus stated:

	From Hitterdahl.	From Rudelstadt.
Sulphur	19·0	19
Copper	69·5	58
Iron	7·5	18
Oxygen	4·0	5
	100·0	100

It is difficult to conceive that this mineral varies so much in its composition as that the copper in one specimen should exceed that of the other in the proportion of 69·5 to 58, or that, as also stated by Klaproth, the quantity of iron in one should be more than twice as great as that in the other specimen; if they had been crystallized, this difference might be supposed to have arisen from a cause already mentioned; namely, that other copper ores are sometimes covered with this.

In order to form some idea of the probable constitution of the variegated copper ore from both these analyses, I shall state what are, I believe, almost universally, or with little variation, allowed to be composition of sulphuret of copper, sulphuret and persulphuret of iron.

By Mr. Chenevix's analysis, native sulphuret of copper consists of

Sulphur	19
Copper	81
	100

According to Dr. Thomson, hydrogen being 1, sulphur is 16, iron 28, and copper 64; and sulphuret of copper is composed of

One atom of sulphur	16
One atom of copper	64
	80

These proportions it will be observed agree almost precisely with those quoted from Chenevix. According to Mr. Hatchett, magnetic pyrites, or the protosulphuret of iron, is composed of

Sulphur	16·06
Iron	27·94
	44·00

This determination agrees also very nearly with Dr. Thomson's numbers, according to which, it is composed of

One atom of sulphur	16
One atom of iron	28
	44

Persulphuret consists of, according to

	Dr. Thomson.	Mr. Hatchett.
Two atoms of sulphur	32	32·16
One atom of iron	28	27·84
	60	60·00

These statements are also very nearly similar.

Putting the oxygen out of the question, it will be seen that the specimen of copper ore from Hitterdahl is stated to consist of 19 of sulphur, 7·5 of iron, and 69·5 of copper. Now in order to take the simplest view of the subject, let us examine whether these quantities are compatible with the supposition that this ore may be a compound of an atom of sulphuret of copper with an atom of sulphuret of iron. According to what has been already stated, 64 of copper combine with 16 of sulphur, or one-fourth of its weight, then 69·5 would require nearly 17·4 of sulphur, which being deducted from 19, the whole quantity, would leave only 1·6 to combine with 7·5 of iron; now the protosulphuret of iron is composed of 16 sulphur and 28 iron; consequently the 1·6 of sulphur would be sufficient for only 2·8 of the 7·5 of iron. It is indeed true, that if we reckon 2·6 of the 4 as sulphur, which

Klaproth has considered to be oxygen, we may regard this ore as a compound of sulphuret of copper and of sulphuret of iron; and it would then appear to consist of four atoms of sulphuret of copper and one atom of sulphuret of iron; thus

Four atoms of sulphuret of copper.	64 sulphur 256 copper
One atom of sulphuret of iron....	16 sulphur 28 iron
	364

According to this, its composition will be

	By theory.	By analysis, substituting sulphur for oxygen.
Sulphur...........	21·98	21·65
Copper...........	70·33	69·50
Iron..............	7·69	7·50
	100·00	98·65
	Loss....	1·35
		100·00

This is unquestionably a possible, but I think not a probable, combination; and that the coincidence is accidental is more likely, because the specimen from Rudelstadt contains the same quantity of sulphur, nearly 12 per cent. less of copper, and about 2½ times more iron.

According to Mr. Chenevix (Phil. Trans. 1804, p. 60), this ore consists of

Sulphur.......................	17 to	25
Copper........................	65	60
Iron..........................	13	15
	100	100

On these analyses I would remark, that in the first statement the sulphur exceeds only by 0·75, the quantity required to convert the copper into sulphuret, without leaving any to combine with the iron; while in the second, the proportions of the constituents are such as to indicate a compound of two atoms of sulphuret of copper and one atom of sulphuret of iron.

From the differences which exist in the proportions of the constituents of this mineral by the eminent analysts named, I was desirous of submitting the variegated copper to fresh examination: for this purpose I employed a specimen from Ross Island, in the lake of Killarney, which appeared to be remarkably pure, of a crystalline structure, although not exhibiting a regular crystalline form, and perfectly unmixed with any other kind of copper ore, to which I am inclined to attribute the variations in the analyses that I have already quoted.

As a preliminary step, I roasted some of the ore until the sulphur was perfectly expelled; the residuum was dissolved in nitric acid to saturation. Water added to the solution occasioned no precipitation, nor did muriate or sulphate of soda. From these experiments I conclude that the ore contained neither antimony, bismuth, silver, nor lead. A quantity of the ore, without being roasted, was dissolved in nitric acid; the solution after being treated with nitrate of barytes, gave no precipitate with nitrate of lead; consequently the ore contains no arsenic.

In order to determine the proportions in which the sulphur, iron, and copper, exist in the ore, I reduced 120 grains of it to powder, and heated it in a retort with dilute nitric acid, adding muriatic acid towards the end of the operation, to prevent the separation of the peroxide of iron.

After the complete conversion of the sulphur into sulphuric acid, I found 0·6 of a grain unacted upon; it consisted of small particles of quartz which had evidently been mechanically mixed with the ore.

To the solution of the ore, nitrate of barytes was added in excess to precipitate the sulphuric acid. The sulphate of barytes was separated by a double filter; dried on a sand heat, after being thoroughly washed, it weighed 216·5 grains. Of this, only 211 grains could be removed from the filter, and they lost 4·8 grains by exposure to a red heat in a platina crucible; consequently 5·5, the quantity left on the filter, would have lost 1·25 grains by similar treatment. The whole quantity of ignited sulphate of barytes amounted, therefore, to 210·45 grains.

To the filtered solution, after the separation of the sulphate of barytes, sulphate of soda was added to precipitate the excess of barytes employed; the solution again filtered was treated with ammonia in excess, which precipitated the peroxide of iron. This, after washing and ignition, weighed 24 grains.

The ammoniacal solution of copper was put into a retort, and evaporated to dryness. The residual mass was dissolved in muriatic acid, and precipitated with excess of potash: the precipitate, which was peroxide of copper, was washed, dried, and ignited; it weighed 91·6 grains.

According to Dr. Thomson, 118 of sulphate of barytes are equivalent to 16 of sulphur; 210·45 will, therefore, indicate 28·5. According to the same authority, 40 of peroxide of iron contain 28 of metallic iron; 24 will, therefore, give 16·8: peroxide of copper is universally allowed to contain one-fifth of oxygen; 91·6 will, therefore, give 73·28. It appears, therefore, that this ore consists of

	In 120 parts.	In 100 parts.
Sulphur	28·5	23·75
Iron	16·8	14·00
Copper	73·28	61·07
Silica	0·6	0·5
	119·18	99·32
Loss	0·82	0·68
	120·00	100·00

What has been already stated as to the composition of the sulphuret of iron and that of copper, will show that 16·8 of iron require 9·6 of sulphur to form the protosulphuret or magnetic pyrites, leaving 18·9 of sulphur to unite with 73·28 of copper. Now as copper combines with one-fourth of its weight of sulphur, 73·28 will require 18·32, leaving an excess of 0·58 of sulphur.

From what has been now detailed, I think it will appear that the variegated copper is a definite compound of one atom of sulphuret of iron and two atoms of sulphuret of copper, or indicated, as already noticed, by one of the analyses of Chenevix, or it may be regarded as consisting of magnetic pyrites and vitreous copper ore. On this view of the subject, its atomic constitution will be as follows:

1 atom of sulphuret of iron 16 + 28 =	44
2 atoms of sulphuret of copper 16 + 64 × 2 =	160
	204

Or it consists of

Sulphur	28·23	23·53
Iron*	16·47	13·73
Copper	75·3	62·74
	120·00	100·00

These quantities, it will be observed, do not differ from the analysis which I have given, more than may be reasonably allowed for the errors of operation.

ARTICLE II.

Meteorological Observations made at Crumpsall, in Lancashire. By Mr. John Blackwall.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Crumpsall, Dec. 17, 1821.*

I LATELY proposed a plan for taking daily observations of the temperature of the atmosphere (*Annals of Philosophy,* vol. ii. p.

297, New Series), which, if generally adopted, would probably, in a great measure, obviate the error and inconvenience arising from the various and unsatisfactory modes of taking such observations, that are practised by the majority of meteorologists.

I now trouble you with observations on the barometer, accompanied with a few remarks, hoping that they may prove useful in pointing out the advantage of establishing a general uniformity in the manner of conducting observations on the daily pressure of the atmosphere. Should they appear suited to promote this desirable object, I shall feel obliged by your giving them publicity. Your obedient servant,

John Blackwall.

A Series of Barometrical Observations made on Oct. 1, 1821, at Crumpsall, in Lancashire, with a View to determine the best Method of obtaining the Extremes and Mean of the Atmospherical Pressure, during the Period of the natural Day.

Hour.	Barometer.	Hour.	Barometer.	Hour.	Barometer.
12 night	29·405	9	29·200	6	29·453
1	29·365	10	29·195	7	29·510
2	29·320	11	29·200	8	29·535
3	29·285	12 noon	29·220	9	29·560
4	29·242	1	29·240	10	29·580
5	29·240	2	29·255	11	29·595
6	29·232	3	29·290	12 night	29·610
7	29·220	4	29·340		
8	29·200	5	29·400		

The methods of taking daily observations of the pressure of the atmosphere which are generally practised, are so very imperfect, and lead to such erroneous conclusions, that the column of observations headed "Barometer," is, perhaps, invariably one of the most defective in every meteorological journal: indeed, when we reflect how little is certainly known respecting the causes that produce the local changes that are almost perpetually taking place in the weight of the atmosphere, and how fluctuating those causes are with which we are acquainted; when we consider also how few observations are usually made in the day, and that still fewer are made during the night, we shall cease to be surprised at the great inaccuracy of barometrical results.

The laws that regulate the temperature of the atmosphere, at least of that region of it about which our inquiries are more immediately concerned, are better understood, and appear to act with much greater regularity than those that influence its pressure. So uniform indeed are their operations, that the maximum of temperature is now known to occur about half-past two o'clock, p. m. in our latitude, and the minimum about half an hour before sun-rise, in the ordinary course of things. A knowledge of these facts would enable those who have the leisure, and are so disposed, to ascertain the daily extremes and means of temperature, with a tolerable degree of precision, if the invention of self-registering thermometers did not offer a much more eligible means of procuring such information; but the case is very different with regard to atmospherical pressure, as it is quite uncertain at what periods the maxima and minima may take place. It, therefore, rarely happens, that the true daily extremes and means of pressure are obtained from the very small number of observations that are made in the course of the natural day by the majority of meteorologists; and it would be difficult, if not impossible, to lay down a plan for conducting such observations, with the instruments that are commonly used for this purpose, that would be sufficiently exact and convenient for general adoption.

These considerations, together with the desire of doing something towards establishing a more regular and efficient mode of observing than any of those which are at present in use, induced me to turn my attention more particularly to the nature of the instruments employed; when a little reflection convinced me that if a good self-registering barometer could be obtained, it would assist me in my project very materially.

Shortly afterwards, having procured a self-registering barometer, I placed it by a common upright one, in a room on the second floor, about 15½ feet from the ground; and after comparing them, and finding that they corresponded exactly, at 12 o'clock on the night of Sept. 30, I commenced the preceding observations, which were made with the common barometer; one or two gentle vibrations being given to the mercury, for the purpose of disengaging it more effectually from the tube whenever an observation was taken.

The weather was stormy, with frequent showers through the day; a strong gale from the W. prevailing till two o'clock, p. m. when it shifted to the NW, from which point it continued to blow with violence till midnight.

The extremes by the common barometer were 29·610, and 29·195, the mean of which is 29·402, the range being ·415, and the mean of the 25 observations is 29·347.

The following tables contain the results of observations taken at the most convenient hours before noon and afternoon.

Results of Two Observations.

Hours.	Maximum.	Minimum.	Mean.	Range.
7 a.m. .... 6 p.m.	29·453	29·220	29·336	·233
7 do. .... 7 do.	29·510	29·220	29·365	·290
7 do. .... 8 do.	29·535	29·220	29·377	·315
7 do. .... 9 do.	29·560	29·220	29·390	·340
7 do. .... 10 do.	29·580	29·220	29·400	·360
7 do. .... 11 do.	29·595	29·220	29·407	·375
8 or 9 do. .... 6 do.	29·453	29·200	29·326	·253
8 or 9 do. .... 7 do.	29·510	29·200	29·355	·310
8 or 9 do. .... 8 do.	29·535	29·200	29·367	·335
8 or 9 do. .... 9 do.	29·560	29·200	29·380	·360
8 or 9 do. .... 10 do.	29·580	29·200	29·390	·380
8 or 9 do. .... 11 do.	29·595	29·200	29·397	·395

*Results of Three Observations.**

Hours.	Maximum.	Minimum.	Mean.	Range.
7 a.m. .... 1 and 6 p.m.			29·304	
7 do. .... 1 7 do.			29·323	
7 do. .... 1 8 do.			29·331	
7 do. .... 1 9 do.			29·340	
7 do. .... 1 10 do.			29·346	
7 do. .... 1 11 do.			29·351	
8 or 9 do. .... 1 6 do.			29·297	
8 or 9 do. .... 1 7 do.			29·316	
8 or 9 do. .... 1 8 do.			29·325	
8 or 9 do. .... 1 9 do.			29·333	
8 or 9 do. .... 1 10 do.			29·340	
8 or 9 do. .... 1 11 do.			29·345	

The correct extremes for the day, and of course the exact range, are not to be found among these results; yet the true mean is very nearly approximated in several instances, and in one or two, it may even be considered as obtained with a sufficient degree of precision; but this near conformity to the mean of the 25 observations, is evidently merely accidental; and when it is recollected at what different hours, and with what various instruments meteorologists take their observations, the bad consequences of the present want of system will be very apparent.

The extremes by the self-registering barometer were 29·610, and 29·190, the mean being 29·400, and the range ·420. Here the extremes and range may be looked upon as correct, but the mean is erroneous, exceeding the mean of the 25 observations, by rather more than 1-20th of an inch. This arises from the variations of the barometer being irregular between the extremes, a much larger proportion of the observations being below the mean found from these extremes than above it. Whenever the mercury moves uniformly up or down through the natural day, it is plain that the mean found from the extremes must coincide with the daily mean; and in almost every other case, there is a greater probability that this mean should be accurate than that the one found from two, three, or even a greater number of observations should be so. It seems then that the self-registering barometer affords a much more certain method of obtaining correct results of the daily pressure of the atmosphere than any other that could be conveniently adopted; but as the foregoing observations and remarks may not appear so conclusive to others as they do to myself, I purpose giving a series of observations made with the common and self-registering barometers in the month of October, which, I trust, will prove to the satisfaction of every one the great superiority of the latter as a meteorological instrument.

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* In consequence of a mistake in the Maximum, Minimum, and Range, which was not discovered till too late for correction, they are omitted altogether.—*Ed.*

Article III.

Meteorological Journal kept at Bushey Heath, in the Year 1821.

By Col. Beaufoy, FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Bushey Heath, Stanmore, Jan.* 7, 1822.

You will oblige me by inserting in the *Annals of Philosophy* a summary of a meteorological table kept by me at this place, and which I believe to be accurate; one day's observation only (the 17th of July) is omitted. The mean monthly heights of the barometer, thermometer, De Luc's hygrometer, together with the quantity of rain and evaporation in inches, are inserted, as well as the mean temperature observed with a Six's thermometer. The altitudes of the barometer and thermometer were taken at nine o'clock in the morning, at which hour the heat of the weather nearly corresponds with the mean temperature of the natural day; the greatest difference in January amounting to 2·83 degrees, the thermometer in the morning being minus that quantity. The rain guage is 16¼ feet above the ground, and 538¼ feet above the sea. This height was deduced from several corresponding barometrical observations made at Bushey, and in the Strand by Mr. Cary, whose instrument is 73 feet higher than the mean level of the sea; and the summit of Bushey Heath 558 feet, or four feet lower than the Signal House at Beachy Head, which I found was elevated 562 feet above low water mark.

On the 25th of last month (December) at half-past twelve in the morning, the mercury in the barometer at this place sunk to 27·600 inches, the night was very dark, with fog and small rain, accompanied by a light wind from the eastward. This unusual depression of the quicksilver, instead of being indicative of a hurricane, or some other convulsion of nature, was followed by a cloudy morning, with a strong wind from the north-west: at nine o'clock the weather cleared up, and continued fine until the going down of the sun, which set in a bank of dense clouds. The next day the wind came round to the east with rain. On the 28th of December at 9^h 30′, p.m. the barometer stood at 27·8 inches, the wind blew very fresh from the SE, and the subsequent day it was light from the NW, with rain. It is remarkable that in the year there has not been a calm day at Bushey Heath, and in the stormy days of November and December, the wind was particularly unsteady; nearly calm at intervals, followed by violent gusts—a proof that the cause of the wind was constantly fluctuating. I remain, dear Sir, truly yours,

MARK BEAUFOY.

Summary of a Meteorological Table.

1821.	Barom.	Ther.	Hyg.	Rain.	Evap.	Mean temp.	N	NE	E	SE	S	SW	W	NW	Var.
Jan. . .	29·420	34·2	81·0	2·115	0·680	37·03	1	8	0	9	0	11	1	1	0
Feb. . .	29·784	36·8	68·2	0·291	1·300	35·09	1	12	0	1	1	7	0	6	0
March	29·174	40·8	70·2	2·692	2·835	42·12	1	2	0	3	1	14	3	6	1
April. .	29·205	48·4	64·6	2·140	3·710	49·81	2	6	1	0	0	14	1	6	0
May . .	29·577	49·4	62·7	1·930	3·690	48·93	0	3	4	1	0	11	1	8	3
June. .	29·606	54·6	62·1	2·147	3·640	54·61	1	19	1	0	1	2	0	6	0
July . .	29·469	58·3	68·3	2·204	3·710	58·59	1	4	2	1	0	15	1	6	0
August	29·499	63·5	69·2	2·316	4·000	63·20	0	5	5	3	0	10	3	4	1
Sept. . .	29·392	58·4	70·7	2·900	3·000	59·51	0	1	0	4	0	13	4	8	0
Oct. . .	29·460	49·8	73·2	3·258	2·030	50·79	0	2	0	7	2	13	1	6	0
Nov. . .	29·338	46·4	75·8	4·542	1·860	46·88	0	1	0	5	0	19	0	5	0
Dec. . .	29·007	41·5	75·2	4·617	1·500	42·47	0	0	1	6	1	16	0	7	0
Mean .	29·411	48·51	70·1	31·152	30·955	49·09	7	63	14	40	6	145	15	69	5

The winds between the cardinal points are described as NE, SE, SW, and NW.

ARTICLE IV.

An Account of some Experiments on the Communication of Magnetism to Iron in different Positions. By the Rev. Baden Powell, MA. of Oriel College, Oxford.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Plumstead, near Woolwich, Dec.* 17, 1821.

THE first idea of the following experiments was suggested to me on reading sometime since a paper by Mr. Scoresby, published in the Edinburgh Philosophical Journal, No. 8, and an abstract of which is also given in the *Annals* for May, 1821. Among other interesting facts, he states, that iron may be rendered magnetic by being bent, scowered, filed, or twisted, in the position of the magnetic axis, or near it. He states, however, nothing more with respect to the degree of magnetism communicated at different inclinations; it is to the determination of this point that my inquiries have been directed; and I conceive I have discovered a simple law by which the increase of intensity thus communicated is regulated, as the inclination varies from the magnetic equator to the axis. I am not aware of any similar law being given by other writers; the paper above alluded to being the only one I know of, which treats at all on this department of the science of magnetism.

The experiments which I have tried have been conducted in a very simple manner; pieces of iron wire, which were previously found to have no magnetism, were fixed at different inclinations to the magnetic equator (every 10th degree); assuming the dip at 70° 30′, according to Mr. Barlow's determination. The apparatus was fixed in the plane of the magnetic meridian; and the wires being fixed firmly at one end were, by means of the other, wrenched or twisted in such a way that they retained their rectilinear form, and their position at the proper angle. The same number of turns in wrenching was given to each piece; and when thus magnetized, their respective intensities were determined by comparing the deviations which they caused on a light magnetic needle, care being taken that they were all placed in a similar position and distance from the pole of the needle. I selected six sets of experiments which I considered as most to be depended on, the mean results of which are as follow:

Inclination of the wire to the mag. equator.	90°	80°	70°	60°	50°	40°	30°	20°	10°	0
Deviations. Mean of 6 sets of experiments.	26° 34′	26° 10′	25° 10′	23° 15′	21° 10′	17° 50′	14° 15′	10° 30′	3° 50′	0

It is obvious that the deviations do not diminish as the inclinations; I, therefore, after several trials, considered the following law as giving a very near approximation to the above results.

tan. D, ∝ sin. I. D = deviation. I = inclination.

This will be obvious by comparing the respective sines and tangents; which are:

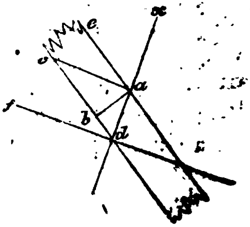
sin. I.	10000000	9848078	9396926	8660294	7660444	6427876	5000000	3420201	1736482
an. D.	5000852	4913386	4698539	4296339	3872058	3217067	2539676	1853390	0670043

In the present experiments, the greatest deviation corresponding to the inc. 90° was, for the sake of convenience of comparison, obtained by placing the wire at such a distance from the pole of the needle as to make it deviate as nearly as possible 26° 34′, and the same distance was kept with the other wires. By examining the tables, it will be seen that the tan. of 26° 34′ is very nearly the half of radius, or sin. 90°, and the tangents which are the nearest to the halves of the other sines given above are those of

26° 13′; 25° 10′; 23° 25′; 20° 58′; 17° 49′; 14° 2′; 9° 43′; 4° 58′;

and this set of arcs differs from the mean of the above experiments by quantities, which are covered by the unavoidable uncertainties both of experiment and observation.

Should the law which I have proposed be considered sufficiently established, I conceive it affords a strong confirmation of the truth of the ingenious theory, proposed by Mr. Christie, respecting the nature of magnetic action (of which he has given an account in the Cambridge Philosophical Transactions, Part I. and in the Edinburgh Phil. Journ. No. 10), when combined (as Mr. C. admits it may easily be) with the theory of M. Ampere concerning the magnetic or electrical currents.



Let us suppose, according to Mr. Christie's idea, magnetic currents in the direction of the dip *x a d*; then *d f* at right angles to this line is the magnetic equator. Let *d c a e* represent a portion of one of the wires magnetized, as in my experiments, of which the thickness is *a b*, and the inclination ∠ *c d f*. Then also the current *x a d* is supposed to be composed of currents perpendicular to its axis; let *a c* be the direction of one of these. The wire is magnetized by imbibing these magnetic currents, being put into a state fit for imbibing them by the torsion; but when magnetized, it also possesses currents perpendicular to its axis; therefore, if it imbibe a current in the direction *a c*, this must be resolved into *a b* and *b c*, of which *a b* is alone effective in producing magnetism. Then it is obvious that *c a* : *a b* :: rad. : sin. inc. and consequently the intensity of magnetism which is measured by the tan. of the deviation produced on the needle, varies as sin. I, which is the law I have deduced from experiment.

This coincidence appears to afford a strong presumption in favour of the actual existence of magnetic currents, or rather systems of magnetic currents (to adopt M. Ampere's idea) in the atmosphere; and for leading us to consider them as the causes by which magnetism is communicated, as in these experiments, or generally by position.

When pieces of iron are placed at different inclinations, it has been found that by mere position they may, after a time, imbibe magnetism. It appears from Mr. Scoresby's experiments, that there are various modes by which this effect may be accelerated, such as torsion, bending, filing, scowering, &c. I have found that a piece of iron may be placed near the poles of a magnet, and remain for some time, without receiving any magnetism; but if, while in this position, any of the above operations be performed on it, it immediately becomes magnetic. I have also found that if a piece of iron wire be bent or twisted into any figure, and in this state be magnetized, and then bent back into an opposite direction, or even simply straitened, its magnetism is either wholly, or very nearly, destroyed. The same thing also occurs if the wire be magnetized when straight, and then bent.

Hence I think we may infer, that an intestine friction of the particles of iron makes it capable of imbibing magnetism; and a similar friction in an opposite direction made after the former produces a contrary effect. What connexion these facts may have with M. Ampere's idea of the spiral currents may be an interesting subject of investigation. At present I will conclude by remarking, that the apparatus used by Mr. Scoresby in his experiments appears, from the description, to be well adapted for experiments of the kind I have described. I, therefore, conceive it not improbable that my ideas may have occurred to him also; but till any more accurate examination of them is made public, I think the present memoir may not be unacceptable to those who are interested in the improvement of this branch of science, which, owing in a great measure to the labours of Mr. Barlow and Professor Hansteen, and the universal interest in it excited of late, both in a theoretical and practical point of view, seems to be advancing with unexampled rapidity; and to promise a rich harvest of discovery, both in application to practical purposes, and in opening new connexions with other departments of science. I am, Sir, yours, &c.

BADEN POWELL.

ARTICLE V.

On the Separation of Iron from other Metals.

By J. F. W. Herschel, Esq. FRS.*

AN easy and exact method of separating iron from the other metals with which it may happen to be mixed, has always been a desideratum in chemistry. Every one conversant with the analysis of minerals is aware of the difficulty of the problem, which indeed is such that, in experiments conducted on any thing like a large scale, it might hitherto be regarded as insuperable. In consequence of this, and of the importance of the inquiry, there is hardly a chemist of eminence who has not proposed some process for the purpose but (with the exception of that which depends on the insolubility of the persuccinate of the obnoxious metal, which I have not tried, and which is too expensive to be resorted to for any but the nicer purposes of analytical research), they are all of them either inadequate to the end proposed, intolerably tedious, or limited in their application. That which I have now to propose, on the other hand, is liable to none of these objections, being *mathematically* rigorous, of general application, and possessing in the highest degree the advantages of facility, celerity, and cheapness. It is briefly this:

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* From the Philosophical Transactions, for 1821, Part II.

The solution containing iron is to be brought to the maximum of oxidation, which can be communicated to it by boiling with nitric acid. It is then to be just neutralized *while in a state of ebullition*, by carbonate of ammonia. The whole of the iron to the last atom, is precipitated, and the whole of the other metals present (which I suppose to be manganese, cerium, nickel, and cobalt), remains in solution.

The precautions necessary to ensure success in this process are few and simple. In the first place, the solution must contain no oxide of manganese or cerium above the first degree of oxidation, otherwise it will be separated with the iron. It is scarcely probable in ordinary cases that any such should be present, the protoxides only of these metals forming salts of any stability; but should they be suspected, a short ebullition with a little sugar will reduce them to the minimum. If nitric acid be now added, the iron alone is peroxidized, the other oxides remaining at the minimum.* Moreover, in performing the precipitation, the metallic solution should not be too concentrated, and must be agitated the whole time, especially towards the end of the process; and when the acid reaction is so far diminished that log-wood paper is but feebly affected by it, the alkaline solution must be added cautiously, in small quantities at a time, and in a diluted state. If too much alkali be added, a drop or two of any acid will set all right again; but it should be well observed, as upon this the whole rigour of the process depends, that no inconvenience can arise from slightly surpassing the point of precise neutralization, *as the newly precipitated carbonates of the above enumerated metals are readily soluble, to a certain extent, in the solutions in which they are formed* (though

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* Dr. Forchhammer, in a paper recently published in Thomson's *Annals of Philosophy*, contends that the proto-salts of manganese are absolutely void of colour. To this I can only say, that I have not succeeded in depriving the muriate of its pale rose colour by any length of ebullition with sugar or alcohol, after which, however, not a trace of deutoxide could be detected in it. I cannot help regarding the process here proposed for freeing manganese from iron as preferable to that of Dr. F.

perfectly neutral). In the cases of cobalt and cerium, this re-dissolution of the recent precipitate formed by carbonate of ammonia is very considerable, and a solution of either of these metals, thus impregnated with the metallic carbonate, becomes a test of the presence of peroxide of iron, of a delicacy surpassing most of the reagents used in chemistry, the minutest trace of it being instantly thrown down by them from a boiling solution, provided no marked excess of acid be present. To be certain, however, that we have not gone too far, it is advisable, after separating the ferruginous precipitate, to test the clear liquid, while hot, with a drop of the alkaline carbonate. If the cloud which this produces be clearly re-dissolved on agitation, we may be sure that only iron has been separated. If otherwise, a little acid must be added, the liquor poured again through the filter, so as to wash the precipitate, and the neutralization performed anew.

The precipitation of iron above described seems at first sight to result from a double decomposition. Were it so, the principle of the method would be merely a difference of solubility in the carbonates of iron and the other metals, and as such would have no claim to be regarded as rigorous. Such, however, is not the case. The iron is not separated in the state of a carbonate, but of a subsalt, or a simple peroxide, the whole of the carbonic acid escaping with effervescence at each addition of the alkali. The phænomenon turns on a peculiarity in the peroxide of this metal, in virtue of which it is incapable of existing in a neutral solution at the boiling temperature. If we add an alkaline, earthy, or metallic carbonate by little and little to a *cold* solution of peroxide of iron, the precipitate formed is redissolved with effervescence, readily at first, but gradually more and more slowly, till at length many hours, or even days, elapse before the liquid becomes quite clear. Meanwhile it deepens in colour till (unless much diluted) it becomes dark brown or red. If the addition of the carbonate be carried as far as possible without producing a permanent precipitate, the solution is perfectly neutral, and continues clear at a low temperature for any length of time. In this state it may be evaporated to dryness in *vacuo*, and the residue (which *does not effervesce* with acids) is still soluble in water without letting any iron fall, and so on as often as we please.

The compound thus formed is, however, far from permanent. It is in fact in a state of tottering equilibrium, which a very slight cause is sufficient to overset. Supposing the point of saturation to have been exactly attained, the addition of an extremely small quantity *more* of the alkaline solution is sufficient to determine the separation of the whole, or nearly the whole, metallic contents; and if the solution operated on be pretty concentrated, it fixes after a longer or shorter time into a stiff and almost solid coagulum. Again, if to the coagulum so formed, a quantity equally inappreciable of the original ferruginous solution be added, it gradually liquefies, and after some time is completely redissolved (forming no inapt representation of the celebrated imposture of St. Januarius's blood).*

A similar change is produced by an increase of temperature. If we heat a solution exactly neutralized as above described, it speedily grows turbid, deposits its ferruginous contents in abundance, *and at the same time acquires a very decided acid reaction.* The acid so developed holds in solution a portion of oxide, but if the neutralization be performed afresh *while hot*, this separates entirely, and the liquid after filtration has no more action on gallic acid, ferrocyanate, or sulphocyanate of potash, than so much distilled water.†

It is not my object in this paper to enter into any minute detail of the nature of the persalts of iron, a subject not nearly exhausted, and which want of leisure alone has prevented my entering upon, but merely to point out the practical application of this one of their properties, to an important object in analysis. The principle here developed furnishes a ready method of detecting the minutest quantities of other metals in union with iron, and, therefore, cannot but prove of important service in various cases where this metal constitutes the chief ingredient in the substance examined, as in meteoric iron, the various natural oxides of this metal, &c. &c. I will exemplify this in one or two instances.

36·00 grains of meteoric iron (furnished me by the kindness of Dr. Wollaston) were dissolved in dilute nitro-sulphuric acid, leaving behind a minute quantity of a brilliant black powder, which, however, dissolved by digestion in nitromuriatic acid, and appeared only to contain an excess of nickel. The solutions were mixed, and being neutralized at a boiling temperature by carbonate of ammonia, and the iron separated, a green solution remained. Into this, when boiling, a drop of persulphate of iron being let fall, was immediately precipitated in the state of subsulphate, which, being separated, the solution was boiled with excess of caustic potash till all smell of ammonia disappeared. Oxide of nickel separated, which, collected and strongly ignited, weighed 4·65 grains, or 12·92 on the hundred, which (taking the atom of nickel to weigh 30, and that of oxygen 8, hydrogen being unity) gives 10·20 per cent. for the contents of the specimen analyzed in metallic nickel.

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* The phænomenon described in the text appears to me to differ from ordinary precipitations and solutions, in the small proportion between the precipitant and the precipitate, the solvent and the matter dissolved. I can call to mind but one instance of so small a quantity of matter operating a chemical change on so large a mass, viz. the decomposition of oxygenated water by fibrin and other animal substances. The action seems to be propagated from particle to particle. Whether the superabundant oxide of iron be retained in solution in a state at all analogous to that of the oxygen in Thenard's experiments, might possibly deserve consideration.

† It was in 1815, in the analysis of a specimen of the gold ore of Bakebanya, given me for that purpose by Dr. Clarke, that I first remarked the separation of oxide of iron from a clear neutral solution by mere elevation of temperature, and attributed it to the presence of an oxycarbonate capable of subsisting in a low temperature, but decomposed by heat. That this is not the true explanation is already shown, and I have considerable doubt of the existence of a percarbonate of iron at *any temperature.*

The most elegant mode of exhibiting the experiment is, perhaps, the following:—Having rendered a solution of protosulphate of iron rigorously neutral, by agitation with carbonate of lime and filtration, dissolve in it a small quantity of chlorate of potash (a salt perfectly neutral). The solution when raised to ebullition is peroxidized, a quantity of subsulphate precipitates, and the supernatant liquid is found decidedly, and even strongly acid.

100 grains of titanious iron from North America, being dissolved in muriatic acid (after the requisite ignition with potash) were treated (after separating the titanium) with excess of carbonate of lime, and filtered. The excess of carbonic acid being expelled, ammonia was added, and a small quantity of a white precipitate fell, which speedily blackened in the air, and proved to be mere oxide of manganese, uncontaminated by iron, and amounting to half a grain.

Manganese has been suspected in various species of cast iron; and though Mr. Mushet's experiments go to prove that it does not usually enter in abundance, they can hardly be regarded as establishing the fact of its absence. It might not be uninteresting to resume the investigation with the aid of a mode of analysis so well adapted to experiments on a large scale, as I have no doubt that, with proper care, one part in a thousand, or even less, of manganese might be insulated from iron.

The separation of iron from uranium cannot be accomplished by the process above described, that metal possessing a property analogous to that which forms the subject of this paper. By inverting the process, however, we shall succeed even here. A mixed solution of iron and uranium being deoxidized by a current of sulphuretted hydrogen, and then treated with an earthy carbonate, the iron passes in solution, while the uranium separates. This difference in the habitudes of the two oxides of iron presents us in fact with a kind of chemical dilemma, of one or the other of whose horns we may avail ourselves in any proposed case. In studying the habitudes of uranium, however, I have met with some anomalies which require further investigation. Zirconia too might probably be freed from iron with equal facility by a similar inversion of the process; but this I have not yet had an opportunity of trying satisfactorily.

J. F. W. Herschel.

Article VI.

Results of a Meteorological Register kept at New Malton, Yorkshire, in the Year 1821. By Mr. James Stockton.

1821.	Barometer.						Thermometer.				Winds.											Weather.			Rain.
Months.	Mean.	Maximum.	Minimum.	Range.	Spaces described in inches and parts.	Numb. of changes.	Mean.	Maximum.	Minimum.	Range.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Var.	Brisk.	Boisterous.	Hail.	Snow.	Rain.	Quantity in inches, &c.
January	29·704	30·88	28·62	2·26	6·21	12	36·840	52	20	32	6	6	1	0	4	8	3	3	0	2	2	0	2	5	1·54
February	30·110	30·64	29·07	1·57	6·33	17	35·160	49	20	29	4	4	3	0	0	6	8	1	2	1	0	1	1	1	0·26
March	29·297	30·22	28·70	1·52	11·05	17	41·115	53	29	24	3	1	3	2	4	11	4	3	0	7	4	1	2	13	3·40
April	29·366	29·98	28·33	1·65	9·91	18	49·683	69	33	36	2	5	1	0	8	5	7	0	2	2	3	4	0	10	2·08
May	29·649	30·15	28·80	1·35	6·14	16	49·370	68	32	36	6	3	1	2	3	1	8	0	4	2	1	4	2	13	3·04
June	29·970	30·38	29·45	0·93	3·25	16	53·400	71	40	31	18	8	0	0	0	0	1	0	3	0	0	0	0	4	0·80
July	29·687	30·18	29·10	1·08	6·00	14	58·200	73	38	35	2	5	2	0	4	5	5	2	2	2	1	0	0	15	2·28
August	29·700	30·08	28·84	1·24	4·75	14	61·112	78	40	38	2	6	2	2	3	9	4	1	2	2	0	0	0	12	1·80
Sept.	29·533	30·04	28·80	1·24	6·61	20	57·333	72	44	28	0	3	0	2	8	7	6	2	2	7	2	0	0	14	2·88
October	29·631	30·21	28·40	1·87	8·64	14	49·258	63	37	26	2	0	1	1	4	19	2	1	1	2	2	0	0	10	2·26
Nov.	29·394	30·20	28·45	1·75	14·33	24	43·733	59	30	29	2	0	0	2	12	9	4	0	1	8	10	0	0	15	3·25
Dec.	29·005	30·20	27·38	2·82	14·38	19	39·693	51	30	21	1	1	1	4	10	4	7	2	1	9	9	0	0	19	5·37
Annual means, &c.	29·587	30·88	27·38	3·50	97·60	201	47·908	78	20	58	55	42	15	15	60	84	59	15	20	44	34	10	7	131	28·96

ANNUAL RESULTS.

Barometer.

	Inches.
Highest observation, Jan. 23. Wind, N.	30·880
Lowest ditto, Dec. 26. Wind, S (continuing 14 hours)	27·380
Range of the mercury	3·500
Mean annual barometrical pressure	29·587
Greatest range of the mercury in December	2·820
Least ditto in June	0·980
Mean annual range of ditto	1·606+
Spaces described by ditto	97·600
Total number of changes in the year	201·000

Six's Thermometer.

Greatest observation, Aug. 23. Wind, SE.	78·000
Least ditto, Jan. 2 and 3, wind, N; and Feb. 26, wind, E	20·000
Range of the mercury in the thermometer	58·000
Mean annual temperature	47·908
Greatest range in August	38·000
Least ditto in December	21·000
Mean annual ditto	30·416

Winds.

	Days.
North and East	70·000
North-east and South-east	57·000
South and West	119·000
South-west and north-west	99·000
Variable	20·000

Rain, &c.

	Inches.
Greatest quantity in December	5·370
Least ditto in February	0·260
Total amount for the year	28·960

OBSERVATIONS.

Pressure.—The most prominent features which present themselves, and the most worthy of remark, are the great elevation of the barometer in January, and its unprecedented depressions in December, the greatest of which, and the minimum for the year, occurred near midnight on the 24th, and continued until two, p. m. the 25th, attended with a most violent gale from the south, thunder and lightning, and torrents of rain, amounting, with what had fallen during the previous night, to nearly *three* inches. On the 29th, the barometer again fell to 27·73, after which, it rose rapidly. From the 16th to the 31st, it never attained 29·00, though the changes in its direction were almost daily, and frequently considerable.

Temperature.—The mean annual temperature, which is 1° above that of the preceding year, and is owing to the mildness of the autumnal and winter months, fully compensated for the decrease from the usual averages experienced in May, June, and July, which were the only months below the means of the corresponding periods in 1820.

Wind.—The prevailing winds are again SW. and W. The N. and S. ones are nearly equal, and the NW. and SE. exactly so. The strongest winds have blown from the S. particularly towards the close of the year.

Rain.—The amount of rain, which has annually and gradually decreased since the wet year of 1816 is less than that of the preceding one, though the last two months have nearly brought up the general average. If the rain be taken from the last quarter of the moon, commencing the 16th ult. up to the same time of the present period (the 15th) the total amount exceeds six inches and a half, a most unusual quantity for these parts.

New Malton, Jan. 15, 1822. James Stockton.

Article VII.

Analysis of Two Finnish Minerals. By P. A. Von Bonsdorff, Ph. D. of Abo. (Extracted from Memoirs presented to the Academy of Sciences in Petersburgh.)

I. *Steinheilit, or Dichroite, from Orrijärvi.*

This mineral occurs in the copper mine of Orrijarvi, in the parish of Kisko, in Finland, and has for a long time been considered as blue quartz; it is found sparingly, and is accompanied with common quartz, greyish talc, and yellow copper ore.

The colour of this mineral is either light or dark Berlin blue, and sometimes, though rarely, it is nearly colourless. Those fragments which are pure and of a dark colour, exhibit two different colours very distinctly, like the dichroite from Spain and the East Indies. In one direction the colour is a deep clear blue, and in the other light grey, and sometimes so light as to be nearly colourless; it is translucent; the lustre of the fragments is glassy; it is hard, giving sparks plentifully with steel. According to Count Steinheil, it occurs in four, six, and eight-sided prisms; the crystals are large, and generally incrusted with talc, which renders it difficult to measure the angles: the specific gravity is 2·603. Exposed to the heat of the blowpipe, it becomes paler, and at a higher temperature the thin edges are difficultly fused. It dissolves in borax and microcosmic salt, exhibiting, while cooling, the appearance of iron; it does not combine with soda; with the solution of cobalt, it gives a brown colour, verging to blue on the fused edges. By exposure to a high temperature, the loss amounted to 1·65 per cent. and the experiment was repeated with scarcely any variation. The analysis was performed as follows:

a. 296·2 parts of fragments of this mineral, of a violet blue colour, were reduced to powder, and levigated in a calcedony mortar, with the addition of water. The powder being dried had not increased in weight, proving that it had gained nothing from the mortar. The powder was heated with 1000 parts of carbonate of potash in a platina crucible, the mass was dissolved in dilute muriatic acid, and evaporated to dryness in a platina dish. It was again digested in muriatic acid, and the silica left weighed, after ignition, 146.

b. The muriatic solution was decomposed by ammonia, and gave a precipitate which, after washing, was boiled with solution of potash, and then filtered. From this, muriatic acid and carbonate of ammonia separated alumina, which, after washing, was ignited and weighed; it was then treated with sulphuric acid, which left 1·4 of silica; the sulphuric solution upon the addition of potash gave crystals of alum, which contained 96·5 of alumina.

c. The brown precipitate which remained undissolved by the potash was dissolved in muriatic acid, mixed with a little nitric acid, and heated to ebullition. The solution diluted with water was neutralized with ammonia, and precipitated with succinate of ammonia: the precipitate after combustion in an open platina vessel gave 14·8 of peroxide of iron.

d. The solution freed from iron gave 1·0 of alumina with carbonate of ammonia, was then evaporated to dryness, and the muriate of ammonia being separated by heat, it was decomposed by carbonate of potash, and gave a precipitate which after ignition weighed 2·0 parts, and by sulphuric acid, 1·9 of magnesia was dissolved, and 0·1 of oxide of manganese was left.

e. The solution remaining after precipitation with ammonia, treated with carbonate of potash, heated to ebullition, evaporated to dryness, and again dissolved in water, gave a precipitate which, when ignited, weighed 29·8 parts.

Diluted sulphuric acid left 0·7 of silica, and dissolved 29·1 parts, which were magnesia. This magnesia, and that above obtained, were found to be pure by means of a solution of cobalt, which imparted its red colour, and by sulphuric acid, which yielded pure sulphate of magnesia.

The result of this analysis is as follows:

Silica	49·95	containing	25·11 of oxygen
Alumina	32·88		15·35
Magnesia	10·45		4·04
Peroxide of iron	5·00		1·53
Oxide of manganese	0·03		
Volatile matter	1·65		
	99·96		

When the composition of this mineral is considered with relation to the electro-chemical theory, and the doctrine of definite proportions, it is evident that the quantities of oxygen in the silica, alumina, and magnesia, are nearly in the proportion of 1·4 and 6, and consequently we might express the mineralogical formula by $M S^2 + 4 A S$; but as the peroxide of iron can only exist in combination with the silica, and as M. Mitscherlich* has proved that the peroxide of iron gives the same crystalline forms by combining with electro-negative bodies as alumina does, the composition of this mineral will be more properly expressed by the following formula $M S^2 + 4 \left\{\frac{A}{F}\right\} S$, which, when the quantity of oxide of iron remains unchanged, gives the calculated result of the composition of this mineral as follows:

Silica	49·93
Alumina	32·60
Magnesia	10·82
Peroxide of iron	5·00

It will be observed that these proportions agree very nearly with the actual results of the analysis.

II. *Malacolit from Tammare, in Finland.*

During a mineralogical tour, I found this mineral in an abandoned lime quarry, at the village of Tammare in the parish of Hvittis.

It occurs in large masses, accompanied with calcareous spar and noble serpentine. Its colour is white, sometimes greenish white; it is translucent; the lustre vitreous; not very hard, giving but few sparks with steel. It has a laminated structure; the angles of the fragments are similar to those of the common malacolit. Its specific gravity is 3·256.

Before the blowpipe it melts *per se*, with slight effervescence, into a translucent glass. It is dissolved by borax, microcosmic salt, and soda, and forms with them a clear glass. With solution of cobalt, it gives a red colour at the fused edges, indicating the presence of magnesia.

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* See Annales de Chimie et de Physique, tome xiv. p. 172, Sur la Relation qui existe entre la Forme Crystalline, et les Proportions Chimiques. Par E. Mitscherlich.

The analysis was performed as follows: A portion of the mineral reduced to fine powder was ignited with three times its weight of carbonate of potash, and treated, as already described in the former analyses, with muriatic acid, and left pure silica. The muriatic solution gave a small quantity of precipitate with ammonia, which was separated into alumina and peroxide of iron: the remaining solution gave a precipitate with oxalate of ammonia; the oxalate of lime was washed, dried, decomposed by heat, and left carbonate of lime.

The solution was then precipitated with carbonate of potash at a boiling heat; the precipitate obtained was ignited and weighed; by solution in sulphuric acid, it gave a small quantity of sulphate of lime, and the dissolved portion consisted of pure magnesia. The mineral lost 0·32 per cent. by a red heat.

The results of this analysis were as follow:

Silica............	54·83	containing	27.58 of oxygen
Lime	24·76	....	6·95
Magnesia	18·55	....	7·18
Alumina.........	0·28		
Oxide of iron.....	0·99		
Volatile matter....	0·32		
	99·73		

We find from this analysis, that the quantities of oxygen in the magnesia and lime are very nearly equal, and that the oxygen of the silica is four times that of each of the other earths. This mineral is consequently composed of one atom of magnesia, one atom of lime, and four atoms of silica, and its mineralogical formula will be $C\ S^2 + M\ S^2$.

ARTICLE VIII.

A Demonstration of a Proposition from Simson's Euclid, p. 301. By Mr. James Adams.

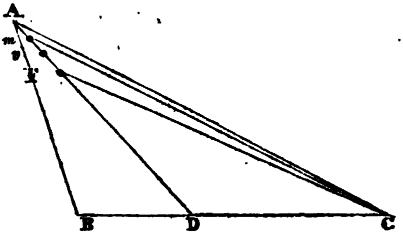
(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, Jan.* 12, 1821.

THE insertion of the following proposition and demonstration in the *Annals of Philosophy,* when convenient, will oblige

Your humble servant,
JAMES ADAMS.

Proposition.—To demonstrate that two sides of a triangle that is inclosed within another, may together be greater than any two sides of the triangle that includes it, in any ratio which is *less than that of two to one.*



Demonstration.—Let ABC be a scalene triangle whose shortest side is A B, in the side B C; take C D equal to A B, and join A D; make D F also equal to A B, and divide A F into indefinitely small equal parts, such as A *m*, *m v*, &c. Join *m* C, F C; then (20 . 1 *e*.) will $Am + mC > AC$, much more will $mF + mC > AC$, and $mD + mC > AB + AC$. Now suppose A C and the angle B A C to remain constant while A B, B C, vary; then if A B decrease, C B will increase, for $AB + BC$ will always be greater than A C; and when A B and its equals, C D, D F, become indefinitely small, the points D and F will approach to the point C, and the point B to the point A as their limits, but to which they never can arrive as long as the triangles B A C and D *m* C have any magnitude: hence the variable lines B D, B C, *m* D, approach to the fixed line A C as their limit; so that the difference between them may at length become less than any assignable line. If the points D and F be conceived actually to coincide with the point C, and the points B and *m** with the point A, the triangles B A C and D *m* C will cease to exist, for their equal bases A B and C D will vanish together; then $mC + mD$ would become $AC + AC = 2AC$, and $AC + AB = AC + 0 = AC$; therefore, the ratio of $mC + mD$ to $AB + AC$, may be any ratio, *less than two to one.*

Corollary.—Neither an isosceles triangle standing on its shortest side, nor an equilateral triangle, will answer the conditions of the proposition, because the straight line A D, drawn within the triangle A B C, will be *less* than either A B, or its equal A C.

It is stated at page 301, before quoted, that Pappus Alexandrinus has demonstrated this proposition in book the third of his mathematical collections, which I have never seen, neither do I know that a demonstration of the property has been published elsewhere.

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* In the former part of the demonstration, A F is supposed to be divided into indefinitely small equal parts, and D F is supposed to be diminished continually; therefore, A F would, in consequence, increase, and become any length less than A C; but since *any* given quantity divided by an indefinitely *great* quantity, will produce an indefinitely *small* quantity, A *m* may be considered indefinitely small.

ARTICLE IX.

Historical Sketch of Electro-magnetism.

(To the Editor of the *Annals of Philosophy.*)

MY DEAR SIR,

I REGRET that circumstances have occurred which have prevented me from completing the sketch of the history of electro-magnetism, of which you have already received a part. Much has been done in this new branch of science since last April, up to which time my brief account goes, but I am not so circumstanced as to be able to give a fair account of it. As you wish for the theoretical notices I had got together, I send them herewith, leaving it with your discretion to use them as you think fit.

I am yours very truly, M.

(*Concluded from* vol. ii. p. 290, New Series.)

Having, in the previous pages, endeavoured to give you such an account of the experimental results as have been obtained by the labourers in this new branch of science, I will now, in as brief a manner as possible, state the theoretical views taken of them by different philosophers as far as I can understand them. The first attempt at a theoretical explanation of the phenomena, which deserves attention, is that of M. Oersted. It cannot be doubted for a moment by any one who has read the papers of this philosopher both on the discovery and prior to it, that his theory rather led to the experiments, than the experiments to the theory. Chance indeed seems to have had very little to do with the discovery except in retarding it, for the thoughts were conceived, and the experiments devised, some time before they were made. Notwithstanding all this, I have very little to say on M. Oersted's theory, for I must confess I do not quite understand it. Before the year 1807, a work was published by M. Oersted, entitled, "An Inquiry into the Identity of Chemical and Electrical Forces," and the eighth chapter of it is occupied in considering the identity of the magnetic and electric powers. In this work, M. Oersted proposed to try whether electricity the most latent, has any action on the magnet, and appears to have considered the two powers as identical.

When, however, the experiment had been successfully made, M. Oersted was enabled to give a more defined form to his theory, and his first paper* concludes with an hypothesis that will, he thinks, readily explain all the phenomena. When a wire is made to connect the two poles of a battery so as to discharge the electricities of those poles, an effect is supposed to take place in the wire, dependent on the union of the electricities, called the electric conflict; and it is this effect, or action, or state of the electricities that is considered capable of affecting the magnetic needle, and changing its direction.

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* See *Annals of Philosophy*, xvi. 276.

The electric conflict acts only on the magnetic particles of matter. All non-magnetic bodies appear penetrable by the electric conflict, while magnetic bodies, or rather their magnetic particles, resist its passage, and are, therefore, moved by the impetus of the contending powers. The electric conflict is not confined to the conductor, but is considerably extended through the circumjacent space, otherwise it could not act on the needle at a distance. It also performs circles, for, without this condition, M. Oersted says, it seems impossible that any one part of the uniting wire when placed below the magnetic pole should drive it towards the east, and when placed above it towards the west; but it is the nature of a circle that the motions in opposite parts should have an opposite direction.

M. Oersted then adds, that all the effects on the north pole mentioned in his experiments, may be easily understood by supposing that negative electricity moves in a spiral line, bent towards the right, propelling the north pole, but not acting on the south pole. The effects on the south pole are explained in a similar manner, if to positive electricity be ascribed a contrary motion, and power of acting on the south pole, but not on the north.

The theory of M. Oersted, therefore, seems to require that there be two electric fluids; that they be not either combined or separate, but in the act of combining so as to produce an electric conflict; that they move nevertheless separate from each other, and in opposite spiral directions, through and round the wire; and that they have entirely distinct and different magnetical powers; the one electricity (negative) propelling the north pole of a magnet, but having no action at all on the south pole; the other electricity (positive) propelling the south pole, but having no power over the north pole.

I have before said, that I am not able to comprehend the whole of the Professor's statement, and, perhaps, therefore, ought not to send you any account of it. It is to be hoped, however, that this celebrated philosopher will shortly develope the principles more at large, which have already led him to the results he has published; and there can be no doubt that in pursuing them he will arrive at other results as new to the world, as important to science, and as honourable to himself, as those he has already made known.*

The experiments made by M. Berzelius have been mentioned in a former part of this letter. They are contained in a letter to

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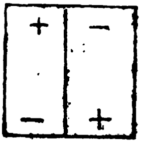
* See *Annals of Philosophy*, ii. 321, New Series.—*Ed.*

M. Berthollet published in the Annales de Chimie, xvi. 113, and are accompanied by some theoretical notions very different to those of M. Oersted. Instead of using a round wire to connect the two poles of the battery, M. Berzelius employed bands of tin, and parallelopipeds, and concludes that they present the magnetic phenomena under better circumstances for observation than the round wire. His conclusion is, that the internal magnetic state of a transverse section of the wire may be represented by two magnets placed with their opposite poles together as in the figure; so that if the wire used be square, it will then be a metallic parallelopiped through which the electric current moves, each of its angles will be a magnetic pole, equal in extent to the length of the parallelopiped, through which the current is passing: the opposite angles will be magnetic poles of the same kind; while those which terminate the same face will have different poles. Hence in passing a needle round the wire, four poles should be found, a north, a south, a north, and a south.*



M. Berzelius remarks also, that it appears each electricity (for he supposes two) is represented in the wire by its own magnet, and that each has its analogous magnetic pole turned to the same side as regards its direction. It is evident, he says, that the ordinary magnetic phenomena differ from those of a current in this, that in the latter case there is a double and inverse polarity, while in common magnets there is only simple polarity, and though the double magnetic polarity may be readily imitated artificially, there are no means known of imitating by electricity, the simple magnetic polarity.

M. Berzelius thinks that this exposition explains all the phenomena that have yet been observed, and will explain all those that shall be; for, he says, it is sufficient to foretell all those of which the conducting body is in this state susceptible. He considers M. Ampere as quite wrong in his conjectures, and the hypotheses of M. Oersted, though ingenious, yet very improbable. It is, however, probable that M. Berzelius has been himself too hasty in his conclusions. The state of the wire, indicated in the section above, is utterly incompatible with the experiments of M. Oersted and others, as may readily be seen by reference to fig. 2, 6, 7, 8, 9, 11, of Pl. IX, vol. ii. *Annals*, New Series, and to the phenomena they are intended to illustrate. Indeed it is only necessary to experiment with a view of ascertaining the four supposed poles in the angles of a square wire, and it will be immediately found that instead of any particular angle exhibiting a constant polarity, it will present the phenomena of either a north or south pole as the needle approaches from the one or the other side towards it. There can, however, be but little doubt that M. Berzelius will correct his opinions, and contribute to the advancement of this branch of science, by something worthy of his great name.

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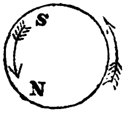
* *Annals of Philosophy*, ii. 227, New Series.

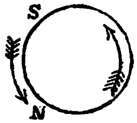
Among the names of those whom I have had occasion to note at different times as occupied in endeavouring to give such an account of the principles of electro-magnetic phenomena as should form a correct theory, or at least such a statement of the laws which govern them as should account for the phenomena, is that of Dr. Wollaston.

Dr. Wollaston has not himself, that I know of, published any thing; but a statement appeared in the Quarterly Journal of Science, x. 363, which has his name to it, and is to be assumed, therefore, as containing his opinions. The high value of this philosopher's opinion is well known, and I should withhold a very important part of this sketch, if I were not to copy all the little that comes with such authority.

" The phenomena exhibited by the electro-magnetic or conjunctive wire may be explained upon the supposition of an electro-magnetic current passing round the axis of the conjunctive wire, its direction depending upon that of the electric current, or upon the poles of the battery with which it is connected.

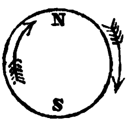
Dr. Wollaston.

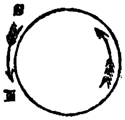




" In the above figures, such a current is represented in two sections at right angles to the axis of the wire, when similarly electrified, from which it will be apparent that the north and south powers meeting will attract each other.

" In the following figures, the sections of the wire are shown





dissimilarly electrified, by which similar magnetic powers meet, and consequently occasion a repulsion."

M. Schweiger, of Halle, has also proposed a theory which he thinks more explanatory of the new phenomena than that of M. Oersted. The latter indeed he opposes as insufficient to account for many of the effects, and inconsistent with others. The only account I have yet seen of M. Schweigger's theory is in the Bibliotheque Universelle, March, 1821, p. 199, where it is stated that to explain the phenomena, he supposes two magnetic axes to exist in each transverse section of the conducting wire, the axes being perpendicular to the direction of the current, and the one above being in one direction, the one below in the opposite direction. This opposition in the direction of the magnetic current in each of these axes is necessary, because of the absolutely inverted manner in which the phenomena are presented when the needle is above and below the wire.

It is difficult to understand how the above theory is to explain the phenomena described by Oersted, but it would not be fair here to give an opinion on its merits, as the account is not taken from the original paper, but from an abstract drawn up in another language.

The Marquis Ridolfi appears to have formed the idea that electricity may be a compound of magnetism and heat, and many experiments are described in the Bibliotheque Universelle, Feb. 1821, p. 114, &c. made with a view of separating electricity into these elements, or of composing it from them. No experimental proofs of the correctness of the opinion were obtained.

Of all the theoretical views that have been given of electro-magnetical phenomena, those by M. Ampere are the most extensive and precise, and have been tested by the application of facts and calculation very far beyond any of the rest. Indeed it is these alone among all those that have been given to the public, which deserve, if any do, the title of *A Theory*. If I had professed to send you any thing more than a sketch of electro-magnetism, I should have been afraid to touch this theory, but as it is, I trust that M. Ampere will excuse the imperfections he may see in the following account, if for nothing else, yet for the humble professions of this letter.

M. Ampere commences by assuming the existence of two electric fluids, according to the theory which is now general, I believe, in France. There appears to be no doubt about his meaning on this point, for though he uses the term electricity very frequently, and in a way which might be understood, perhaps, as applying equally either to a particular state of a body, or to a particular fluid existing among its particles, yet by the use of the term *electric fluids* in one place, and by the mention of electric currents as currents of matter, it is nearly certain that M. Ampere means to speak of electricity as consisting of two distinct fluids, which, though the one is called positive, and the other negative electricity, are to be considered as equally positive in their existence, and possessed of equal powers.

The voltaic battery is considered as an instrument possessing the power of conveying one of these electricities to the one end, the other to the other end. That which goes to the zinc end of the battery is called positive electricity; that which goes to the copper end negative: these names being retained, it may be presumed merely in deference to custom, and not because they have any reference to particular qualities of either the one or the other fluid.

When a metallic wire is made to touch the two poles of the voltaic battery, being a conductor of electricity, it carries off the two fluids; but the battery having within itself the power of continually conveying fresh portions of the fluids to the two extremities, the first portions that are removed by the wire are succeeded by others, and thus currents are produced, which are constant as long as the battery remains in action, and the poles continue connected by the wire. Now as it is in this state that the wire is capable of affecting the magnetic needle, it is very important for the exact comprehension of the theory that a clear and precise idea of its state, or of what is assumed to be its state, should be gained, for on it in fact the whole of the theory is founded. Portions of matter in the same state as this wire, may be said to constitute the materials from which M. Ampere forms, theoretically, not only bar magnets, but even the great magnet of the earth; and we may, therefore, be allowed to expect that a very clear description will first be offered of it. This, however, is not the case, and is, I think, very much to be regretted, since it renders the rest of the theory considerably obscure, for though certainly the highly interesting facts discovered by M. Ampere could have been described, and the general laws and arrangements both in conductors and magnets stated with equal force and effect without any reference to the internal state of the wire, but only to the powers which experiment proves it to be endowed with, yet as M. Ampere has chosen always to refer to the currents in the wire, and in fact founds his theory upon their existence, it became necessary that *a current* should be described.

At p. 63, vol. xv. Annales de Chimie, M. Ampere, while speaking of the battery and connecting wire, says, it is generally agreed that the battery continues to convey the two electricities in the two directions it did at the moment the connexion was first completed; "so that a double current results, the one of positive electricity, the other of negative electricity, parting in opposite directions from the points where the electro-motive action exists, and reuniting in that part of the circuit opposed to those points." This reunion would, of course, take place in the wire, and one may be allowed to ask, whether the magnetic effects depend on it, as M. Oersted seems to think, who calls it the electric conflict, and also what becomes of the electricities that accumulate in the wire. But from other parts of M. Ampere's memoirs, a very different idea of the electric currents may be gained; the one electricity is considered as continually circulating in one direction; while the other electricity circulates and moves in a current in the opposite direction, so that the two electricities are passing by each other in opposite directions in the same wire and apparatus.

Without, however, dwelling on the state of the wire when thus circumstanced, M. Ampere is content, in order to avoid confusion, when speaking of the direction of the electrical currents, to wave attention to the two, and to speak as if there were but one only, which is to be called the electrical current, without any reference to positive or negative, and which is considered as moving in the battery from the copper to the zinc end, and in the wire from the zinc to the copper end. It is evident that thus modified, the existence of the current, and its direction, are assumed simply for the convenience of having something to which the direction of the electro-magnetical motions may readily be referred; and, consequently, when thus spoken of, no reference is made to the way in which the double current exists in the wire, or to the cause of the production of magnetism by it.

In the historical sketch I have already given you of the facts as they were discovered, I mentioned that M. Oersted first ascertained the mutual action of the wire and the magnetic needle. He showed that the apparatus had power over the needle only when the connexion was completed, consequently the electricity must be in progressive motion, or forming a current, as M. Ampere states, before it can become magnetic. M. Ampere, then, discovered the fact that two electrical currents (using the word in his own sense) were capable of acting on each other, and producing entirely new electrical phenomena. This discovery was noticed in the former part of this letter,* and it was mentioned that when the currents were in the same direction, they attracted each other; when in different directions, they repelled each other. These attractions and repulsions differ entirely from those exhibited by electricity in a state of tension, as may be seen by referring back to the account given of them. M. Ampere nevertheless considers them as belonging to the electricity, but only when it moves in currents. They are, he thinks, dependent on certain properties which these currents possess, and are not produced by the action of any magnetic or other fluid which the electricity has set at liberty. Electricity, when accumulated, has the power of causing certain attractions and repulsions which are called electrical; when in motion it has the power of causing certain other attractions and repulsions; namely, those in question.

Having then ascertained these new properties of electric currents, M. Ampere, in the progress of his reasonings, reverted back to Oersted's experiment, and removing one of the currents, he substituted a magnet in its place. The results were the same as before; the attractions and repulsions were of the same kind, and took place in the same manner; so that the effects which were known to be electrical with the two wires, were produced, when in place of one of them a magnet was used: only, the distribution of the powers in the magnet seemed to differ from that in the wire or current; for that power which is exhibited by one side of the wire is concentrated in one end of the magnet, and that power exhibited by the other side of the wire in the other end.

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* *Annals of Philosophy*, vol. ii. p. 275, New Series.

On taking away the remaining wire, and substituting a second magnet for it, the two acted in the usual manner; but the action was found to be analogous to that of two electrical currents. So that M. Ampere was forced by his experiments, and the view he had taken of them, to conclude, that all the attractions, whether excited by two wires, a wire and a magnet, or two magnets, were purely electrical, and, in fine, that all magnetic phenomena are occasioned by electric currents.

Taken in this point of view, electricity and magnetism are identical, or rather, magnetic phenomena are a series of electrical phenomena. Hence magnetism should form a branch of electricity under the head of electrical currents; but before we dispose of it in this premature, though convenient, manner, we should endeavour to state what the arrangement of electrical currents are which M. Ampere has found it necessary to assume to account for the various known phenomena of magnetism.

The arrangement of magnetic power in a conducting wire is so different to that in a magnet, that it is not at first very evident how the one may be considered as convertible into the other. Currents of electricity, according to the theory, were essentially necessary to the production of magnetic phenomena, but where are the currents in a common magnet? It was a bold thought to say they actually existed in it, but M. Ampere has ventured the idea, and has so arranged them, theoretically, as to account for very many magnetic phenomena.

A magnet, M. Ampere says, is an assemblage of as many electric currents moving in planes perpendicular to the axis, as there may be conceived lines, which, without cutting each other, form closed curves; for, he says, it seems impossible to him from the simple consideration of the facts, to doubt that there are really such currents round the axes of magnets; and magnetization consists, he says, in an operation by which there is given to the particles of steel the property of producing in the direction of the currents before spoken of, the same electromotive action which is found in the voltaic pile, the electric calamine of mineralogists, the heated tourmaline, and even in the pile formed of moistened paper, and discs of the same metal at different temperatures.

With regard to the extent of the curves which these currents travel through, the theory has not yet decided whether it relates to the whole magnet, or to the particles of which it is formed. If a section of a magnet perpendicular to its axis be conceived, the currents situated in it may either be concentric, in which case they will vary gradually in extent, or they may exist round each particle, in which case they are of uniform size, but very minute. It appears from calculation that either of these arrangements would account for the phenomena. M. Ampere is, I believe, inclined to adopt the latter.*

Conceiving a magnet then to be formed in this way of electric currents, and reverting to the experimental results obtained by the action of a wire and a magnet on each other, if one end of the magnet be presented to one side of the wire, it will attract it; if to the other side, it will repel it. The reason according to the theory is evident: the currents pass in different directions on the two sides of the magnet up on one side down on the other. When that side is towards the wire in which the currents move in the same direction as in the wire, attraction takes place; when the opposite side is towards the wire, repulsion takes place, because the currents are in opposite directions. If the magnet be turned round, and the other pole be brought near the wire, the direction of the currents in the magnet will be turned also, and motions opposite to those which before took place will now occur, because the place of the similar and dissimilar currents has been changed.

In consequence of the idea which had been formed of a magnet as an assemblage of electric currents in planes perpendicular to the magnetic axes, M. Ampere endeavoured to obtain an imitation by forming a spiral or helix of wire, and passing a current of electricity through it. As the electricity traversed the spirals, it would nearly resemble the different currents in the magnet; and the effect of the obliquity of the spirals was counteracted by returning the wire from the extremity down the axes of the helix. This instrument has been described before,† and the similarity of the effects produced by it to those of the magnet stated.

It would lead me far beyond my original intention were I to extend further on this part of M. Ampere's theory, nor is there any occasion; for I am sure all those who are anxious to understand or pursue the subject, will think it necessary to read M. Ampere's papers; and for those who may think a sketch sufficient, I have already said enough. Let us, therefore, notice very briefly that philosopher's opinions on terrestrial magnetism.

Naturally led by his elaborate views to substitute terrestrial magnetism for the magnet he had previously used in experiments on the wire, M. Ampere was induced to suspend a circle very delicately, in hopes the earth's magnetism would make it traverse; for as according to his theory, wire and magnets moved each other, not by any supposed pole or point of attraction and repulsion, but by the attraction and repulsion of the currents passing through them, he hoped to be able to make a current move also by those he assumed to exist in the earth. The success of this experiment has been related,‡ and was certainly sufficient to make the author trust very confidently to a theory which had guided him so safely to such novel and important results.

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* Journal de Physique, xcii. 163.

† *Annals of Philosophy*, ii. 281, New Series.

‡ Ibid, ii. 279.

The traversing of the curve by the magnetism of the earth added another argument to those in support of M. Ampere's theory. If the experiment had not succeeded, the distinction between the curve and the needle would have been fairly urged against the theory; as it succeeded, it admits of being adduced as another proof that currents in curves such as those M. Ampere assumes to exist in the magnet, are sufficient to account for the phenomena presented by it. But the important conclusion M. Ampere arrives at from it is, that the magnetism of the earth is itself caused by currents of electricity, which, moving from the east towards the west round the globe are at right angles to the magnetic meridian. These currents, if they exist, are compared to those which would be found in a voltaic battery if its two extremities were made to meet. There is nothing probably in the globe which can be compared to the continuous conductor formed by the metallic wire, but M. Ampere has shown that the battery itself is magnetic; and he supposes it probable that the arrangement of the materials of the globe may be such as to constitute a battery existing like a girdle round the earth, which, though composed of comparatively weak elements, is sufficiently extensive to produce the effects of terrestrial magnetism. Its irregularity in that case would account for the distorted forms of lines of similar variation, and the changes that take place in it would explain the change of the direction of the needle. Some general action, however, is supposed to exist which aids in producing the currents of electricity, and in a direction approaching parallelism with the equator; and the variation is supposed to depend on the progress of oxidation in the continental regions of the earth.

The diurnal variation is considered as dependent on the diurnal change of temperature in the superficial electro-motors of the globe. The various strata of magnetic materials are considered as so many voltaic piles.

Supposing that electric currents actually exist in the masses of matter which form the planetary and stellar globes, M. Ampere suggests the possibility that they may sometimes be so powerful as to make the heat which is necessarily excited rise to ignition. In this case, a permanent incandescence with a brilliant light would be produced without either combustion or loss of substance. "May we not suppose," says M. Ampere, "that the opaque globes are so only because of the small degree of energy in the electric currents established in them, and find in the more active currents the cause of the heat and light of those globes that shine by themselves."

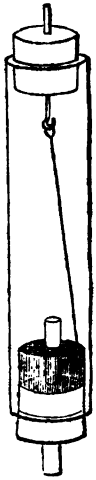
Such, Sir, is the sketch I must beg you to accept of M. Ampere's theory. I need not again apologize for its imperfections, but refer, as to an easy remedy, to the philosopher's own papers in the Annales de Chimie. I must again say, that having assumed the existence of two distinct electric fluids, and the identity of electricity with magnetism, I think the first part of the theory by no means sufficiently developed. M. Oersted has, in this respect, aimed at more perfection than M. Ampere; with what success, it is not necessary for me to determine.

[To the historical sketch of electro-magnetism with which I have been favoured by my anonymous correspondent, I shall add a sketch of the discoveries that have been made by Mr. Faraday, of the Royal Institution; the memoirs which contain the account of these very important experiments are contained in the 12th volume of the Quarterly Journal.—*Ed.*]

Mr. Faraday's attention was first directed to the verification of the results obtained by previous experimenters as to the attractions and repulsions of the needle by a connecting wire: in attempting this, he ascertained that the position of the needle with respect to the wire greatly modified the effects produced; he ascertained that the apparent attraction of the needle on one side, and its consequent repulsion on the other, did not occur under all circumstances, but that accordingly as the wire was placed nearer to, or further from, the pivot of the needle, attraction or repulsion was produced on the same side of the wire: this will, perhaps, be more clearly understood in the author's own words: "If the wire be made to approach perpendicularly towards one pole of the needle, the pole will pass off on one side in that direction which the attraction and repulsion at the extreme point of the pole would give; but if the wire be continually made to approach the centre of motion, by either the one or the other side of the needle, the tendency to move in the former direction diminishes; it then becomes null, and the needle is quite indifferent to the wire; and ultimately the motion is reversed, and the needle powerfully endeavours to pass the opposite way."

From the facts which have been now stated, Mr. Faraday concludes, that the centre of magnetic action, or true pole of the needle, is not placed at its extremity, but in its axis at a little distance from the extremity and towards the middle; that this point has a tendency to revolve round the wire, and necessarily, therefore, the wire round the point; and as the same effects in the opposite direction take place with the other pole, it is evident, in the opinion of Mr. F. that each pole had the power of acting on the wire by itself, and not as any part of the needle, or as connected with the opposite pole. The attractions and repulsions he considers merely as exhibitions of the revolving motion in different parts of the circle.

It will not be necessary to follow Mr. Faraday through all the difficulties which he had to contend with, or to describe every ingenious form of apparatus by which these difficulties were overcome. The annexed cut will exhibit one of the modes which he employed to exhibit the motion of a wire round a magnetic pole. Place a portion of mercury in a tube closed below by a cork, and fix a small magnet so that one pole shall project above the surface of the mercury. Take a piece of clean copper wire about two inches in length, amalgamate the two ends, form a loop at one end, and at the end of another piece of wire form another loop, by which hang the first piece; this affords free motion, and the amalgam allows good contact, fix this over the magnet, so that the end of the moveable piece shall just dip into the mercury; then connect the mercury with one pole of a voltaic combination, which is readily done through the magnet, and the wire with the other; and the moveable part will immediately revolve round the magnetic pole, and continue to do so as long as the contact is continued. On bringing the magnetic pole from the centre of motion to the side of the wire, there was neither attraction nor repulsion; but the wire endeavoured to pass off in a circle, still leaving the pole for its centre, and that either on one side or the other according to circumstances.



All the directions of the motion are reducible to two; when a current of electricity passes through the wire, the north pole rotates in one direction, and the south in the other. Suppose a watch lying on the table, and let its face be considered as the mercury, and the pivot the north pole of a magnet; a wire dipping into it being negative below, and positive above, would pass round the pole in the direction of the hands of the watch; if the connexion be reversed, or the magnetic pole changed, the motion will be the reverse of the hands. If the wire be made fast, and the pole move round it, the motion is similar, and in the same direction.

Our limits will not allow us to describe the numerous and highly curious and interesting experiments which Mr. Faraday has made with the poles and wires, having one or more of each, and arranged in different ways. The results of some of these experiments were, that needles, instead of being attracted by their poles, were attracted by their centres. Needles were not attracted merely by the wire, but on arriving at it, still endeavoured to continue their course in the direction in which they had begun it, and on the wire being removed from the one side to the other of them, so as to obviate the mechanical impediment it offered, they move on as at first, being apparently repelled: no attraction was observed to exist between a pole and a wire: all these phenomena are referable to the revolving motion.

There are some results relating to the theory of M. Ampere which appear very extraordinary. It is first noticed that there is a similarity between the natural magnet and the helice electro-magnet of M. Ampere. It is also observed that owing to the form of the imitation, and the properties of the wire, such a helix must exhibit the properties of the two sides of the wire in a separated state at its extremities. Mr. Faraday then proceeds to notice the polarity of a circle formed from a portion of connecting wire, and of the helice: this he considers to be merely the result of rotation. In revolving round a wire, the pole describes a circle in a plane perpendicular to it, and it moves with equal force in each part of the circle. These circles it would describe round each successive portion of the wire; but let the wire be considered as bent into a ring, and it is evident these circles recede from each other in the external part, while in the centre of the ring they converge together and accumulate in one spot just as would happen with the spirals of a bell-spring if it were bent into a ring; consequently the powers which move the pole are most energetic in the centre of the ring into which the wire is formed; and though the movement has the appearance of attraction on one side of the ring, and of repulsion on the other, it is always that of rotation; hence the production of what are called poles; and the transition from the poles of a ring to the poles of helices are clearly made out.

This explanation of the electro-magnet is then followed by experimental illustrations, and it is afterwards compared with the common magnet; the accordance Mr. Faraday appears to think as great; but he nevertheless mentions some differences; among them are the following: The similar poles of magnets repel at most distances, but if brought very near to each other, they attract; this attraction is not strong, and it differs from that of dissimilar poles in not inducing any neutralizing effect. Two dissimilar poles will take up a certain quantity of iron filings when separate; when together, they will not take up nearly so much, but two similar poles will take up as much, and even more, when together, than when separate. This effect is not produced by the helice magnet. Is it not probable that the effect mentioned by Mr. Faraday may result from the reciprocal inductive action of the magnets upon each other?

No success attended any of the attempts to render the properties of common cylinder magnets similar to those of the helice magnets. M. Ampere's experiment of directing a curved conducting wire by the magnetism of the earth was repeated with success.

The influence of the earth's magnetism in producing the effects which had been obtained by a common magnet was next endeavoured to be ascertained by Mr. Faraday; at first he was unsuccessful; but since his first paper was published, a second has appeared in the form of a note at page 416 of the Institution Journal, which contains facts meriting a much more conspicuous place. As the action of the magnetic pole on the wire was always independent of the axis joining the extremes or poles of the magnet; it was concluded that in those motions which would probably be produced by the earth, all consideration of the magnetic axes might be omitted for the time, and the pole considered as a point, the position of which is indicated by the dipping needle. Mr. Faraday does not appear to lay much stress on the existence of magnetic poles in the earth, but is rather inclined to consider them as apparent only, and the result of an action analogous to that of the ring before spoken of; but he seems to assume the dipping needle as indicating the resultant of all the terrestrial magnetic forces; and, therefore, as the datum on which to commence, Mr. Faraday has made a mistake in giving the dip as 72° 30′; it is only 70° 30′. The motions, however, do not regard the quantity of dip so as to be confined to a certain range, but probably occur in any part of the earth.

Judging from the former experiments, the results expected were that a connecting wire would always move laterally, and in a plane at right angles to the dip: this requires the wire to be perpendicular to the dip; if removed from the perpendicular a little way, it would still, however, move, though with diminished force. To get this result experimentally, a horizontal piece of wire was suspended from the ceiling by a silk thread, its ends dipped into mercury in two basins, and these were connected with the voltaic apparatus; the wire immediately moved laterally, and that in every azimuth, and the direction of the motion was precisely that described in the former experiments. Thus when the wire was E. and W. the E. end to the zinc, and the W. end to the copper plate, a single pair of plates being used, the motion was towards the N.; when the connexions were reversed, the motion was towards the S. When the wire hung N. and S. the N. end to the zinc plate, the S. end to the copper plate, the motion was towards the W. when the connexions were reversed towards the E. and the intermediate positions had their motions in intermediate directions.

An apparatus was made use of in another experiment resembling that described for the revolving motion, but larger and more delicate, and the moveable wire was made to form a greater angle with a perpendicular than that formed by the dipping needle. In these circumstances, the moment the communication was completed, revolution began, and continued by the magnetic force of the earth alone on the wire.

Mr. F. deduces from these experiments, the cause of the direction taken by Ampere's curve. Considering it as a polygon of an infinite number of sides, he shows that the attempt of those sides to rotate by terrestrial magnetism would place the curve in the position, M. Ampere found it to take in his experiments. Mr. Faraday concludes this part of his note by stating his expectation "that in every part of the terrestrial globe, an electro-magnetic wire, if left to the free action of terrestrial magnetism, will move in a plane (for so the small part we can experiment on may be considered) perpendicular to the dip of the needle, and in a direction perpendicular to the current of electricity passing through it."

An expectation was entertained, in consequence of this law, that where the dip was small, a difference in the weight of an electro-magnetic wire might be perceived when the current passed through it in different directions. In endeavouring to estimate if the difference were perceptible in these latitudes, a very remarkable effect was observed. On suspending a piece of wire from a lever, and letting very fine wires dip from it into two cups of mercury, it apparently became lighter every time the electric current was passed through it either one way or the other. This effect was at last found not to be a real alteration in the gravity of the wire, but to be an affection of the mercury, with which it was in contact. The wires when dipped into the metal drew up a little elevation around them owing to the cohesive attraction of the mercury. On close inspection, it was observed that every time the connexions were completed, these elevations were diminished, so that in fact the wire was lightened of a portion of the weight before attached to it; on breaking the connexions the elevations resumed their original bulk. Hence when electricity passes from a fine wire into mercury, or from mercury into a fine wire, an effect is produced equivalent to a diminution of the cohesive attraction of the mercury. Whether it is really such a diminution, or is due to some other cause, remains to be determined.

In concluding this imperfect sketch of the labours of Mr. Faraday in this new and interesting branch of science, we earnestly recommend him to continue his researches on a subject which he has so ably illustrated and enriched by discoveries that are in the highest degree curious and important.

Article X.

Reply to B. M. By John Murray, FLS. MWS. &c. &c.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Surry Institution, Jan.* 12, 1822.

I am a stranger to the name which the letters B. M. are proposed to adumbrate. If *truth* be the object of this writer, why does he blush to own it? Is science to be a masquerade, and its friends appear in false or fictitious characters? An honest man ought to be ashamed of such a contemptible subterfuge—"*talia fures.*" It is something to grapple with a noble enemy even should we fall in the contest, but it is neither expedient nor profitable to exchange "thrusts" with a shadow. I have thus premised, because I have laid it down as a fixed principle never to notice any attack upon me by anonymous personages, and this must be my apology for the undisturbed silence I shall in future preserve in similar cases.

The observations introduced into the pages of the Philosophical Magazine comprise only a *very few* selected from *very many* experiments on the subject in question, and I drew my inferences from the *combined aggregate*, and not from individual and insulated phænomena. The *inefficacy* of steel or iron filings in cases of poisoning by muriate of mercury as pronounced by B. M. and their efficacy as inferred by analogy, on my part, is the subject in both cases of *mere opinion*. Here then we stand on equal ground. The subject of poisons has received much of my attention; and when my *experiments* come to be detailed, perhaps it will be seen that I shall have laboured to better purpose in this interesting field than B. M.

This writer has inferred, that the steel he used had no magnetism in its composition, because it did *not attract iron filings;* but how numerous are the instances where magnetism obtains, and this property is absent. De la Rive's floating annulus is highly magnetic, but I have not observed any tendency to attract iron filings. Perhaps the authority of Sir H. Davy may weigh with B. M. "The only proof of the magnetic powers of electricity passing through such a fluid was afforded by its effect upon the magnetized needle." But it is needless to extend observations of this description, when we know position, juxta position with a magnet, filing, hammering, scowering, twisting, &c. all communicate magnetism to steel or iron; and it is *more than probable* that magnetism is never absent from iron, and that to this may be ascribed the action of ferruginous bodies on the magnet in which the attractive effect seems mutual.

I was not ignorant of the action of muriate of mercury or nitrate of silver on steel which B. M. has presumed to suppose (after *he himself* seems to have been set right with respect to the latter) by referring to the authorities he thus superfluously quotes.

The precipitation of one metal by another, as of copper by iron, silver by copper, &c. has been ascribed to voltaic influence by Von Grotthus, Sylvester, Donovan, &c.; while the principle is generally recognized by philosophers; and, *prima facie*, is it not reasonable to suppose that the separation of every metal from its combination with every acid whatever (at least to the great extent I have proved it to be) is to be attributed to the magnetism of the iron or steel, an influence or power possessed almost exclusively by them? Where am I to find recorded that iron separates silver from a solution of the *acetate*, or *platinum from the nitromuriate*, &c. Even the extensive and almost unbounded power which iron exercises over the domain of metallic salts would itself be a *discovery*. The cause of the effect is the only legitimate subject of question. As to the phænomena of the bars immersed in phosphorous acid (B. M. seems to have used *phosphoric* acid), I have simply stated the fact as it occurred to me, and became the object of my senses; and the same observation applies to the *platinum wires*. This last was witnessed by *several persons* as well as myself, though it is neither for nor against the general issue; neither do I mean to insist upon this phenomenon, which may have been the incidental effect of an *impurity* in the solution of nitrate of silver; but that the phenomenon did occur as I have described it, I do most unequivocally assert.

I have nothing to do with the mere hypothetical part of B. M.'s paper; I must, however, stigmatize the expression "fallacious" (which he, however, with one exception only, has proved experimentally correct), and "inferences unwarranted by facts," to be precipitate, rude, and ungentlemanly.

I have the honour to be (in haste), Sir,
Your obedient servant,
J. MURRAY.

ARTICLE XI.

On CADMIUM, *and the Habitudes of some of its Ores, showing the Means of detecting the Presence of the Metal in English Ores of Zinc.* By E. D. Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, &c.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cambridge, Jan.* 21, 1822.

IT is now nearly two years since I communicated to the Editor of the *Annals of Philosophy* a discovery (since fully confirmed by much abler chemists than myself) of the presence of *Cadmium* in the *Derbyshire* ores of *zinc.*[*] As I believe that the complete reduction of this metal and its exhibition in the metallic state have not yet been accomplished by any chemist in Great Britain (all the specimens of it that have been seen, coming to us from the continent), I shall again call the attention of your chemical readers towards the means we possess of effecting this object, especially as I have since discovered *cadmium* in a greater variety of substances than I was at first aware of, and have it in my power thereby greatly to facilitate the means of procuring it. In one single instance, and almost accidentally, I

[*] See *Annals of Philosophy*, xv. 272, Art. V. dated Feb. 18, 1820.

have myself succeeded in the revival of *cadmium*, so as to witness its beautiful silvery aspect; but I have never been able to obtain the metal in any quantity, and it is only within the last week, owing to the kindness of *Dr. Wollaston*, who presented me with a lamina of pure *cadmium*, as he received it from Professor *Stromeyer*, that I have had the satisfaction of examining this curious substance. From the appearance exhibited by this specimen, it has the colour and lustre of *silver*, and has all the pliability of a piece of tin foil. Its other characters in the metallic state are known, and, therefore, need not be mentioned.

Before I proceed to any further account of its ores, I will just mention two experiments for its revival, which seem to be attended with success. The first I have once before mentioned; it consists in filling a glass test-tube (which contains a portion of the *oxide of cadmium* adhering to the inner surface) with *hydrogen gas*, and then with a common blowpipe directing the flame of a candle against the tube, so as to give it a red heat. In this experiment, if care be used so as to prevent the breaking of the glass tube, metallic *cadmium* seems apparent in a thin cuticle upon the inner surface of the glass vessel. I have said "*seems apparent,*" because the quantity is too small to allow of any satisfactory examination afterwards, and also because it may be objected that this metallic appearance is owing to the revival, not of the *cadmium* from its oxide, but of another metallic oxide used in the manufacture of the glass. Another way, less liable to objection, relates to an experiment by which I undoubtedly obtained a sight of this metal, although in a quantity so minute as to be hardly visible to the naked eye. It was accomplished in the following manner: A small quantity of the pure *oxide of cadmium* was dissolved in muriatic acid, and a piece of paper being steeped in the solution and dried, was made into a pellet between the fingers, and supported upon a slip of platinum foil before the *blue flame* of the blowpipe. Here as the muriate became concentrated by the burning of the paper, and afterwards decomposed, the *oxide of cadmium* was reduced by the *carbonaceous* matter both of the *paper* and the *blue flame,* and as it began to burn and to exhibit its *reddish-brown protoxide* upon the platinum foil, a small bead of *cadmium* remained upon the surface of the platinum; which, being fixed into the end of a deal splinter, admitted the action of the file, and exhibited the silvery aspect of the pure metal. In this experiment I had no room to doubt of the nature of the result; because the *oxide* which I used came from *Professor Stromeyer* himself, and there was no impurity in any of the substances I had employed. As the temperature at which *cadmium* becomes volatilized is so inconsiderable, it is of course difficult to attempt its reduction by means of heat; the metal burning in the very instant of its revival, unless the greatest caution be observed. The phenomena, however, attendant upon its combustion are among the most remarkable properties of the metal; and they are those of which chemists have availed themselves in detecting the presence of very minute portions of *cadmium* in its various ores. *Berzelius*, in the inestimable volume which he has recently published upon the "*Use of the Blowpipe*,"* seizes with avidity this striking character, and makes it the prominent and discriminating character of the ores of *cadmium*.† But long before the publication of this work of *Berzelius*, and at the time when *Dr. Wollaston*, by his own experiments confirmed the fact of the discovery of *cadmium* in the English ores of *zinc*,‡ this illustrious chemist had already availed himself of the same striking property in the metal. Speaking of the *oxide of cadmium* as obtained from those ores, in a letter which I received from him dated February 14, 1820, *Dr. Wollaston* says, "fixed at the tip of the blue flame, it is gradually reduced, volatilizes, and is carried along the slip of platina, coating it with its peculiar reddish-brown protoxide in a way that cannot be mistaken by one who has once seen it." Indeed so striking is the manifestation of this character in ores containing the most minute portions of *cadmium* that the fact of its presence in the *silicates* and *carbonates of zinc* needs no other test. But there is a method of putting those minerals to the trial of *cadmium* which seems to me preferable to that mentioned by *Berzelius*, who uses *charcoal* for a support. It is simply this: Triturate a portion of the *silicate* or *carbonate of zinc* supposed or not to contain *cadmium*, and place about the tenth of a grain of this powder upon a slip of *platinum foil*. Then direct the *blue flame* of a candle towards it by means of the blowpipe; if any *cadmium* be present, its oxide will be reduced, volatilized, and a protoxide will be deposited upon the surface of the *platinum*, with the peculiar reddish-brown colour before mentioned.

Another mode of showing the presence of *cadmium* in the ores of *zinc*, remarkable for its simplicity and certainty, is also due to *Dr. Wollaston*. This consists in dissolving the *carbonates of zinc*, or gelatinizing the *silicates* in *muriatic acid*, getting rid of the excess of *acid*, and adding distilled water; then removing any metals that *iron* will precipitate, and filtering the solution, which is to be received into a platinum capsule, containing a piece of *zinc*. The *cadmium,* if any be present, will coat over the interior surface of the capsule with a precipitate of a dull leaden hue, and will adhere so firmly as that it may be washed, and thereby freed from any remaining solution of *zinc*. Muriatic acid being now poured into the capsule will dissolve the lead-coloured coating with effervescence, and either the *carbonate of potass* or *caustic potass* will yield a white precipitate, which, by heat before the *blue flame* of the blowpipe, will exhibit the remarkable character already pointed out, as characteristic of *Cadmium.*

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* "De l'Emploi du Chalumeau dans les Analyses Chimiques," &c. a Paris, 1821.

† "Ce phénomène est si marqué dans l'oxide de cadmium, que les minéraux qui comme le carbonate de zinc, contiennent un ou deux pour cent de carbonate de cadmium, étant exposes un seul instant au feu de réduction, déposent à peu de distance de la matière d'essai, un anneau jaune ou orangé d'oxide de cadmium que l'on aperçoit d'autant mieux que le charbon est plus refroidi. Cet anneau se forme bien avant le commencement de la réduction de l'oxide de zinc, et si les flocons de zinc se montrent en même temps, c'est une preuve que l'on a poussé l'insufflation trop loin; mais si l'on ne peut découvrir aucune trace jaune avant que la fumée de zinc commence à former un depot sur le charbon, on doit en conclure que la matière d'essai ne contient pas de cadmium."—(*Ibid.* p. 132.)

‡ Dr. Clarke since writing this article has himself discovered cadmium in the metallic or sheet zinc of commerce. A communication from him upon this subject will appear in our next number.—*Ed.*

As it will not I hope be long before some of the chemists of Great Britain will obtain *cadmium* in the metallic state from the ores which this country affords, I will mention the localities of some of them, and give such a description of the minerals in which I have myself detected the presence of this metal, that there can be no difficulty in meeting with a supply of ore necessary for the experiment.

The *Cumberland Cave,* near *Matlock,* contains both *silicate* and *carbonate of zinc,* and both are *cadmiferous.* I have received from *Professor Sedgwick* of this University, specimens of both those minerals, which he himself brought from that cave. The *carbonate* being the most abundant, I will describe this first. All the *carbonates* and *silicates of zinc* found in the *Cumberland Cave* go by the name of *Calamine,* and are promiscuously sold by the dealers in minerals, either as *electric* or *non-electric Calamine,* just as the name best answers the purposes of sale. Hence arises that confusion in cabinets of mineralogy, whose owners, trusting to the dealers, have not given themselves the trouble to examine chemically the specimens they have bought. Nothing can be more easy than to distinguish between a *carbonate* and a *silicate of zinc,* even when they are not crystallized: both are soluble in acids leaving no residue, but the first effervesces upon the immediate action of the acid; while the second, exhibiting no effervescence, forms, as the solution evaporates before a fire, or over a lamp, in a watch glass, a transparent jelly.

The *carbonate of zinc* of the *Cumberland Cave* is often sold as a *silicate.* It is a very compound mineral, consisting of no less than three distinct varieties of the *carbonate* aggregated into one mass, besides *galena, fluor, quartz, sulphate of barytes,** and other bodies. To speak, therefore, of the *specific gravity* of such a mineral mass would be absurd. Of the three varieties of the *carbonate of zinc* which it contains, all are *cadmiferous.* The *first* is of a honey colour, exhibiting a sparry fracture, and a radiated structure, like *wavellite*. The *second* is a grey stalactite body, externally resembling *chalcedony*, and appears in cavities as if the substance, like tallow or wax, had been melted and flowed over the surfaces of those cavities. The *third* is an earthy, or arenaceous body, of an *orange-brown colour*, wholly soluble, with the most lively effervescence, in *muriatic acid*. All these contain rather less than one per cent. of *carbonate of cadmium;* insomuch that when the mineral has been triturated and exposed upon *platinum foil* to the action of the *blue flame* before the blow-pipe, the proof of the presence of *cadmium* is made instantly apparent by the test already mentioned.

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* Small flattened acicular crystals of the *sulphate of barytes* are seen in the cavities, opaque, and of a white colour. Immersed in *muriatic acid* these crystals effervesce owing to some earthy *carbonate of zinc,* by which they are covered; but they become afterwards transparent, and remain insoluble, exhibiting their true characters before the blowpipe.

In the same cave where this *carbonate* is found, there is found another mineral, called also *Calamine*, which is *cadmiferous;* but which instead of being, like the former mineral, a *carbonate*, is a *silicate of zinc*. This occurs more rarely, but the mineral is crystallized, and very pure, and gelatinizes in *muriatic acid*, in the most perfect and transparent manner, being slowly soluble in that acid without any effervescence.* This mineral presents an aggregation of small crystals, whose forms cannot distinctly be ascertained. Externally it is of a grey colour, and when broken, the interior of the crystals exhibits the sort of radiated or stellar structure, which characterizes one variety of the *carbonate;* and the two minerals so much resemble each other in this respect, and in their lustre and colour, that they may easily be confounded. The *specific gravity* of the *silicate* estimated in pump water at the temperature of 50° of Fahrenheit equals 3·10.

Possibly all the *Derbyshire carbonates of zinc* may contain *cadmium*, as well as some of the *silicates*. This metal appears to me to be so decidedly present in many of the ores used in our manufactories that I have reason to believe it exists also in the *zinc* manufactured from those ores; for if a little sheet *zinc* be scraped with a knife, and the powder placed upon *charcoal* or upon *platinum foil*, and exposed to the blowpipe, the appearance of the "*anneau jaune, ou orangé, d'oxide de cadmium*," mentioned by *Berzelius*† as a test of the presence of the metal is easily manifested. This is a matter which may soon be confirmed or contradicted by a regular chemical examination of the *zinc* of commerce.

Other *carbonates of zinc* containing *cadmium* are those of *Mendip*, in *Somersetshire*, of a dark grey colour, investing cavities, as a stalactite, in masses otherwise of a reddish-brown hue. I have not been able to detect any of this metal in the *carbonates of zinc* from *Holywell*, in *Flintshire*. The specimens which I examined were stalactites, which had coated over the crystals of other bodies, and destroyed them; appearing in hollow pseudomorphose forms. I have also been unable to ascertain its presence in the white botryoïdal *silicates of zinc* from *Hungary*. But as this article has already been extended to a greater length than I at first expected, I shall, for the present, postpone making any further observations.

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* It was found in the *Cumberland Cave* as before mentioned, by *Prof. Sedgwick*.

† See a former note.

EDWARD DANIEL CLARKE.

ARTICLE XII.

Remarks on a peculiar Imperfection of Vision with Regard to Colours. By Whitlock Nicholl, MD. MRIA. FLS. &c. &c.

(To the Editor of the *Annals of Philosophy*.)

SIR,

CASES of imperfection of vision with regard to colours are recorded in the Philosophical Transactions; and similar cases,* which I have reported, have been published in the Transactions of the Medical and Chirurgical Society of London.†

The principal peculiarities in each of these cases are; the confounding red with green, and pink with blue. Since no attempt has (as far as I am acquainted) been made to explain the cause of these peculiarities of vision, I am tempted to hazard some conjectures respecting it.

Before I enter, however, on the subject of these cases, I must premise a few general observations.

Cases occur in which the sensibility of the retina, with regard to light, is above the natural standard. We also meet with cases in which that sensibility is below that standard. In the former cases, a faint degree of light causes the production of that sensation which we term *seeing;* whereas, in the latter instances, a strong degree of light is requisite for the production of that sensation. A retina, then, may be sensible to a powerful degree of light, although it is insensible to fainter degrees of light.

When the retina has been powerfully acted upon by a powerful degree of light, it may become insensible to light; or, if it have been accustomed to the action of strong degrees of light, it may become insensible to fainter degrees of light; whereas a retina, from which all strong degrees of light have been for some time withheld, may become sensible to very faint degrees of light.

When the retina has been powerfully acted upon by one particular set of the prismatic rays, it may become insensible to that set; yet it may continue sensible, or it may be so in an increased degree to the other sets of rays.

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* See two papers, the one bearing date Jan. 1777; the other, May in the same year.

† Vols. vii. and ix.

When the retina has been for some time powerfully acted upon by rays of light which have produced the sensation *seeing red*, that sensation will at length cease, although the action of those rays be continued; yet if at such time the action of those rays be even continued, or if rays of mixed light be admitted to the retina, or if all rays of light be excluded from the retina, the sensation *seeing green* will arise. In like manner, the long continuance of *seeing green* leads to the production of *seeing red*.

When after the long continuance of the sensation *seeing red*, the sensation *seeing green* arises in the manner just described (and *vice versâ*), although the sensation thus undergoes a change with regard to what we term *colour*, it continues precisely the same with regard to what we term *shape*, and *extent of surface;* so that a retina which has become insensible to the action of one of the prismatic rays may still be adequate to all the common purposes of vision, since, although there may be a defect of sensation with regard to *colour*, it does not follow that there is any defect with regard to *shape* and *extent of surface*.

It appears then that there is a certain state of the retina necessary, in order that the mixed rays of light so affect it as to produce vision. It also appears, that there is a particular state of the retina requisite, in order that each separate and distinct set of the rays of light so affect it as to produce a corresponding peculiar and distinct kind of vision. There is, for instance, a state of the retina in which all the rays of light, whether blended or distinct, so affect the retina as to produce vision. Such a state may be termed *general sensibility* of the retina. There is a state of the retina in which all the rays of light, whether blended or distinct, fail to affect the retina in such a manner as to produce vision. Such a state may be termed *general insensibility* of the retina. There is a state of the retina in which a certain set of the prismatic rays (as, for instance, the red rays) fail to produce a corresponding kind of vision. There is also a state of the retina in which only one set of the prismatic rays (as, for instance, the red rays) affects the retina in such a manner as to produce vision. In such cases, there is *partial sensibility* and also *partial insensibility* of the retina.

That state of retina then which fits it for being so acted upon by one particular set of the prismatic rays as to produce vision, does not necessarily fit it for being so acted upon by another set, or by all the other sets, of the prismatic rays.

It appears then that whenever light acts upon the retina in such manner as to produce vision, it produces a certain condition or state of the retina which is essential to the existence of that kind of sensation which is termed *seeing*. It appears also, that in order that each distinct set of the prismatic rays so affect the retina as to produce a distinct corresponding kind of the sensation *seeing*, it is necessary that it produce a distinct corresponding state of the retina, which state is essential to the existence of such corresponding sensation. There is, for instance, one state of the retina which is essential to the existence of the sensation *seeing red;* another state which is essential to the presence of the sensation *seeing yellow;* and so on. So that, when we speak of *seeing* a certain fixed number of *colours*, we imply that the retina can have that number of distinct states produced in it. These states which are essential to the presence of sensation, I have termed *sensual states*.

Let us call that sensual state which produces the sensation *seeing red*, A; that which produces *seeing orange*, B; that which gives rise to *seeing yellow*, C; that which produces *seeing green*, D; that which causes *seeing blue*, E; and that which produces *seeing violet*, F.

It has been already stated, that when the sensation *seeing red* can no longer be produced by the action of rays of light, the sensation *seeing green* may still be produced, or it may arise spontaneously; and *vice versâ*. It follows then that the sensual state of retina, A, is a state different from, or opposite to, the sensual state, D.

It appears then that if a retina be capable of assuming the several states, A, B, C, D, E, F, the individual who possesses that retina may have the several sensations, *seeing red*, *seeing orange*, *seeing yellow*, *seeing green*, *seeing blue*, *seeing violet*, produced. If the retina be incapable of assuming any sensual state, the individual possessing it cannot have vision produced, he will be *blind;* but if a retina be incapable of assuming the state, A, the possessor of it cannot have the sensation *seeing red*, he will be blind *quoad* that sensation, and so with regard to the other sensual states.

The several sensual states of the retina from which the varieties of vision arise are commonly produced by the action of rays of light; but it has been shown that the sensations *seeing red* and *seeing green* may arise in the absence of light. It follows then that the presence of rays of light is not essential to the production of sensual states of the retina, or, in other words, to the presence of various kinds of vision. By whatever means then a sensual state of the retina is produced, by such means will vision be produced; the particular kind of sensation present will depend upon the particular state which is induced; for instance, by whatever means the state, A, is produced, by such means *seeing red* will be produced, and so on.

The sensations *seeing red, seeing yellow*, &c. are then only so far dependent upon the action of light, in as much as such action produces a corresponding sensual state of the retina; for *seeing a colour* is a sensation dependent upon a peculiar state of the retina which is commonly produced by the action of rays of light, but which may arise in the absence of those rays.

The *colour* then which is *seen* in any case is connected with the presence of rays of light only in the relation of effect and cause.

Light is divisible by the prism into several sets of rays, whereof one set excites the sensation *seeing red*, the next *seeing orange*, the third *seeing yellow*, the fourth *seeing green*, the fifth *seeing blue*, and the sixth *seeing violet*. These several sets are distinguished by the names red, orange, yellow, green, blue, and violet rays. The red, yellow, and blue rays are termed primary rays. The orange ray is considered as a compound of the red and yellow rays; the green, as a compound of the yellow and blue rays; and the violet, as a compound of the blue and red rays. The orange, the green, and the violet rays are, therefore, termed compound rays.

Let us suppose a retina to be insensible to red rays. In such a case, it would be sensible to the two other primary rays, namely, to the yellow and to the blue rays. It would be sensible also to the ray compounded of these two rays; namely, to the green ray. The action of light then upon such a retina might produce the sensations *seeing yellow*, *seeing green*, and *seeing blue*. It would not produce the sensation, *seeing red*. And as the orange and the violet rays are each in part compounded of the red ray, the sensations *seeing orange*, or *seeing violet*, would not be produced by the action of light upon such a retina.

Let us suppose a retina to be insensible to yellow rays. In such a case it would be sensible to the two other primary rays; namely, to the red, and to the blue rays, and also to the ray compounded of these two rays; namely, to the violet ray. The action of light then upon such a retina might produce the sensations *seeing red, seeing blue*, and *seeing violet;* but it would not produce *seeing yellow*. And as the orange and the green rays are each in part compounded of the yellow ray, the sensations *seeing orange* and *seeing green* would not be produced by the action of light upon such a retina.

Let us suppose a retina to be insensible to blue rays. In such a case, it would be sensible to the two other primary rays; namely, to the red and to the yellow rays, and also to the ray compounded of these two; namely, to the orange ray. The action of light then upon such a retina might produce the sensations *seeing red, seeing orange*, and *seeing yellow;* but it would not produce *seeing blue*. And as the green and the violet rays are each in part compounded of the blue ray, the sensations *seeing green* and *seeing violet*, would not be produced by the action of light upon such a retina.

If a retina be insensible to the orange ray, it may be so from being insensible either to the red, or to the yellow rays, of which two rays the orange ray is compounded. In the former case, *seeing blue, seeing yellow*, and *seeing green*, may arise: in the latter case, *seeing red*, *seeing blue*, and *seeing violet*, may arise from the action of light. In either case, *seeing blue* may be produced.

If a retina be insensible to the green ray, it may be so from being insensible, either to the yellow, or to the blue rays, of which two rays the green ray is compounded. In the former case, *seeing red*, *seeing blue*, and *seeing violet*, may arise; in the latter case, *seeing red*, *seeing yellow*, and *seeing orange*, may arise from the action of light. In either case, *seeing red* may arise.

If a retina be insensible to the violet ray, it may be so from being insensible either to the red or the blue rays, of which two rays the violet ray is compounded. In the former case, *seeing yellow*, *seeing blue*, and *seeing green*, may arise; in the latter case, *seeing red*, *seeing yellow*, and *seeing orange*, may arise from the action of light. In either case, *seeing yellow* may arise.

It appears then that if a retina be insensible to a primary or to a compound ray of light, it can only be affected by light so as to give rise to the sensations *seeing three prismatic colours*, two of which *colours* will be primary colours, and the third, a colour compounded of these two.

Let us apply the preceding observations to the retinæ of the individuals who are the subjects of the cases alluded to at the commencement of this paper.

By these individuals red is confounded with green, and pink with blue.

We have seen that, in ordinary retinæ, the sensual state A, or that which produces *seeing red*, is different from, and indeed opposite to, that state, D, which gives rise to *seeing green*. It is highly improbable then that in these individuals the red ray and the green ray should excite the same kind of sensual state of retina, or that two different sensual states (as A and D) should be produced by the action either of the red or of the green ray. If then the red ray and the green ray cannot produce the same sensual state, and if two different sensual states (as A and D) be not produced by those two rays, it follows, that in these individuals, one of those rays fails to produce a sensual state of the retina; that, in other words, their retinæ are insensible, either to the red or to the green ray; consequently, that if they have sensation produced by the red ray, they have none excited by the green ray, and *vice versâ*.

We arrive then at this conclusion, that the retinæ of these individuals are insensible, either to the red ray, or to the green ray.

If they are insensible to the red ray, those individuals can, as we have seen, only have the sensations *seeing yellow*, *seeing blue*, and *seeing green*, produced by the prismatic rays.

If they are insensible to the green ray, they may, as has been shown, have the sensations *seeing red, seeing yellow,* and *seeing orange;* or *seeing red, seeing blue,* and *seeing violet,* produced by the action of the prismatic rays.

These individuals confound pink with blue. Pink is a faint shade of red. Now as I have argued that the red and the green rays cannot each affect the retinæ of these persons so as to produce *seeing,* but that their retinæ must be insensible to one of these two prismatic rays, so may I adduce the same arguments to prove that they cannot be sensible both to pink and to blue rays; but that they must be insensible either to pink or to blue rays. If they are sensible to pink rays, we must conclude that they are also sensible to red rays. But if they are sensible to pink, we must suppose them to be insensible to blue; consequently, if they are sensible to red and pink rays, they are insensible to blue rays.

It appears then that the retinæ of these persons are sensible either to yellow, blue, and green rays, or to red, yellow, and orange rays. In either case, their retinæ will be sensible to yellow rays.

We accordingly find that these individuals never confound the sensation which is excited in the presence of the yellow ray, with any of those which are produced in the presence of either of the other prismatic rays. Their retinæ are fully sensible to the yellow ray.

If then the retinæ of these persons be sensible to green and to blue rays, they are insensible to red and to orange rays.

As the green ray is compounded of the yellow and of the blue rays; as the orange ray is compounded of the red and of the yellow rays; as yellow enters into the composition both of the green and of the orange rays; and as the retinæ of these persons are sensible to yellow; it appears that they are insensible either to the red or to the blue rays of light.

If then these persons can have the sensation *seeing red,* they cannot have the sensations *seeing green, seeing blue,* or *seeing violet;* and if they can have the sensation *seeing blue,* they cannot have the sensations *seeing red, seeing orange,* and *seeing violet.* In either case then they must be insensible to the violet rays.

It sometimes happens when the retinæ of ordinary individuals have been for a long time acted upon by strong light, that if the blended rays of light be then thrown upon the retina, the sensation which results from their action is that of *seeing red,* we explain this occurrence by supposing, that in consequence of long exposure to strong light, the retina has become insensible to all the rays, excepting the red rays. The red ray then appears to exert a more powerful influence on the retina than any of the prismatic rays.

Of all the prismatic rays, the red ray is the least refrangible, and the most heating; next in order, in these respects, is the orange ray; and, thirdly, the yellow ray.

From what has been stated then it appears probable that if a retina be insensible either to the red, or to the blue rays, it is insensible to the latter of these rather than to the former.

If then we suppose that the retinæ of these individuals are insensible to the blue rays, they are sensible to the red, to the orange, and to the yellow rays, which are the three most powerful, most heating, and least refrangible, of all the calorific rays.

We arrive then at this conclusion that the retinæ of these individuals are sensible to red and to yellow rays, and to the rays compounded of these two sets of rays, or to orange rays; but that they are insensible to green, to blue, and to violet rays.

I proceed, lastly, to compare the conclusions at which I have arrived with the facts observable in the cases of these individuals. But it must be recollected, that it is impossible to ascertain the *kind* of the sensation *seeing* which any ray of light produces in another person by its action upon his retina. We can only ascertain whether two different rays do or do not excite in him two sensations, which he describes as differing from each other. For instance, it is impossible to ascertain whether the red ray produces in another person the same kind of sensation that it excites in me; but I can ascertain whether the red ray produces in him a sensation different from that which he receives in the presence of any other ray. Both he and I agree to call the sensation which the red ray excites in each of us *seeing red*, but this term does not describe the kind of sensation; it merely denotes that such sensation is different from the sensations which are produced in him by other prismatic rays.

The action of the first set of the prismatic rays upon the retinæ of these individuals gives rise to the sensation *seeing a colour*; and to describe this sensation, they employ the term which others use to denote the sensation produced by the action of that set, they call it *seeing red*.*

The action of the second set of the prismatic rays upon the retinæ of these persons gives rise to the sensation *seeing a colour*, which they describe as being of a kind different from that which arises from the action of the first set of rays; and to describe this sensation, they employ the term which others use to denote the sensation produced by the action of the second set of rays; they call it *seeing orange*.

The action of the third set of the prismatic rays upon the retinæ of these persons gives rise to the sensation *seeing a colour*, which they describe as being different from those which are produced by the first and second sets of prismatic rays; and to describe this sensation they employ the term which others use to denote the sensation arising from the action of the third set of rays; they call it *seeing yellow*.

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* They sometimes call it also *seeing green*, since they use the terms *seeing red* and *seeing green* indiscriminately to denote the same kind of sensation.

It will be recollected that when the sensation *seeing red* has been for a long time kept up in ordinary cases by the continued action of the first set of the prismatic rays, the sensation *seeing green* arises, and *vice versâ*. But (in the case of Mr. Buchanan*) when the long continued action of the first set of prismatic rays had produced the long continuance of *seeing colour* (which he also terms *seeing red*), no other sensation of *seeing a different colour* arose; so that, in his case, the long continuance of *seeing red* does not produce *seeing green*, as it does in ordinary cases.

When the retinæ of these individuals are exposed to the action of the fourth set of the prismatic rays, a sensation *seeing a colour* is present, which they describe as being the same as that which is produced by the action of the first set of prismatic rays. The first and the fourth set of rays do not then produce in these persons two different kinds of sensation. If then the first set of the prismatic rays produces a peculiar corresponding sensation, the fourth set of those rays does not produce such sensation as I have already endeavoured to prove; so that if we admit that these persons can have the sensation *seeing red* produced, we must deny their being able to *see green;* we must suppose their retinæ to be insensible to the green rays; to be, in other words, incapable of assuming the sensual state D; and this inference seems confirmed by the fact of no new sensation arising in Mr. Buchanan after the long continuance of that sensation *seeing colour* which is produced in him by the long-continued action of the red ray upon his retina. We are then, I think, warranted in supposing that these individuals do not *see green*. Why then do they *see red* in the presence of the green ray? As far as regards the green ray, they are in the state of a nyctalopic, whose retina is sensible only to the red rays which are contained in mixed light. They are in a state similar to that of persons whose retinæ have been long acted upon by green rays, and who then have the sensation *seeing red*, although their retinæ are still acted upon by green rays. The retinæ of these persons cannot assume the sensual state D; the presence of the green ray, therefore, does not produce any sensation. As then the sensation *seeing red* arises in the presence of the green ray, and as such sensation can be produced only by the action of red rays, such sensation must arise from the action of the most powerful of the rays which are contained in the *mixed light* to which their retinæ are also exposed; namely, of the red rays; so that when their retinæ are exposed to green rays, since they cannot assume the state D, and as they are sensible only to red, orange, or yellow rays, they are acted upon by the red rays which are contained in the mixed light to which they are also exposed, and the sensual state A arises.

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* Medico-Chirurg. Trans. vol. ix. Part II. The experiment was not made in the other case.

When the retinæ of these individuals are exposed to the action of the fifth set of prismatic rays, the sensation *seeing a colour* arises. This sensation is not the same as either of those which arises from the action of the first, of the second, or of the third set of prismatic rays. They describe it as not being the same as *seeing red, seeing orange,* or *seeing yellow.* Wishing then to distinguish by name the sensation which arises in the presence of the fifth set of prismatic rays, they apply to it the name which others use to denote the sensation produced in *them* by the action of those rays; they call it *seeing blue.* These individuals confound dark blue with crimson. Crimson is a dark shade of red. What they call *seeing blue* is then *seeing a deep dark shade of that colour which is excited by the red ray.* The subjects of the two cases which I have recorded call the crimson curtains of their respective beds *blue* by day-light, and *red* by candle-light. That is, by day-light, they have the sensation *seeing a dark shade of red;* whereas, by candle-light, they have a fainter degree of that sensation, or the sensation *seeing a lighter shade* of that colour. As they use the term *blue* to denote *seeing a dark shade of red,* so do they use the term *light blue* to denote *seeing light shades of red,* which other people call pink, or they indiscriminately use the terms *light blue* and *pink* to denote the same sensation. When then the darker shades of blue are presented to the retinæ of these individuals, as their retinæ are (as I have endeavoured to prove) insensible to blue rays, they will not have the sensation *seeing blue,* but they will be acted upon by red rays of a dark shade, which excite the sensation *seeing dark red.* For if they did not *see any colour* during the presence of dark blue rays, they would *see black,* and seeing only a few red rays mixed with *no colour,* they, in fact, *see a dark shade of red.* Mr. Buchanan informs me that he cannot distinguish *dark blue* from *black* by candle-light, and he says, that what gives him by day the sensation *seeing dusky red* affects him so little by candle-light, that he then scarcely *sees* any *colour.* These persons confound grass green with scarlet, light blue with pink, dark blue with crimson, very dark red with black. The fact then appears to be, that these individuals *see red, see orange,* and *see yellow,* from the actions of the first, second, and third sets of rays respectively; but that, as they advance in the prismatic range, their retinæ are affected only by the red rays of the mixed light to which they are also exposed, they *see* only shades of *red,* varying as they advance towards the most refrangible rays, from a sensation similar to that which is excited by the first set of the prismatic rays to less vivid degrees of the same sensation, until at length no sensual state being produced in the retinæ, they have a sensation similar to that which is present in the absence of all luminous rays; that is, they *see black.* They have sensations of *seeing colour* excited only by red and yellow rays, and by the various compounds of these two rays; and they have distinct sensations produced only by strong rays of these sets of prismatic rays; for all dark shades of red, of orange, of brown, as well as deep green and purple, are by them confounded with black, i. e. with the absence of all *colour.* The subject of one of my cases *saw* but three *colours* when he looked through a prism. Mr. Buchanan says that the rainbow appears to him yellow in the centre, and *blue* at the edges, which latter assertion proves that he uses the terms *red* and *blue* to denote similar sensations. The order of the prismatic rays being as follows: Red, orange, yellow, green, light blue, dark blue, violet. The corresponding order of sensations which arise in these individuals, in the presence of these successive prismatic rays, are as follows: Red, orange, yellow, red, pink, dark red, dark indistinct colour, or black.

The facts observable in the cases of these individuals seem to agree with the conclusion which I arrived at in the earlier part of this paper; namely, that the retinæ of these individuals are sensible to red and to yellow rays, and to the rays compounded of these two sets of rays, or to orange rays, but that they are insensible to green, to blue, and to violet rays.

Some persons possess auditory organs, which enable them to discriminate a great variety of *sounds,* while other individuals who possess great quickness of *hearing* with regard to *sound* in general, are, nevertheless, unable to distinguish any great variety in the *kind* of sounds which they hear. There is *general sensibility* of the auditory nerve required for the production of the general sensation *hearing;* there is a *partial sensibility* of that nerve required for the production of varieties in the *kind* of that sensation. If an auditory nerve be so formed as to be capable of assuming a great variety of sensual states according to the nature of the impressions made upon it, the possessor of that nerve is said to possess a *nice ear,* while he whose auditory nerve possesses only general sensibility is said to have an *unmusical ear.* It is the same with regard to the retina. It may possess general sensibility, enabling its possessor to *see shape* and *extent of surface* accurately, while it is incapable of assuming the usual variety of sensual states, thereby preventing the possessor of it from *seeing* that variety with regard to *colour,* which the possessors of the ordinary kinds of retinæ are enabled to distinguish.

Article XIII.

On Congreve Rockets. By Lieut. R. C. Alderson.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Hull, Jan.* 12, 1822.

I was very much astonished on reading in the 12th number of the *Annals of Philosophy* for December last, under the head of Scientific Intelligence, the very ingenious invention of the application of the Congreve rockets to the whale fishery attributed to Capt. Scoresby of the Fame. As a friend of Lieut. Colquhoun, of the Royal Artillery, the inventor of this mode of destruction to the whale, I cannot help contradicting a statement so erroneous. Lieut. Colquhoun having applied to the son of Capt. Scoresby, of the Fame, who commanded a fishing ship which sailed from Liverpool, to take him out for the purpose of trying the effect of the rockets on the whale, and finding that that gentleman had previously engaged to take out Capt. Manby on a different plan, was recommended by the son to the father Capt. Scoresby of the Fame, at Hull, with whom Lieut. Colquhoun and two artilleryman sailed; the result of the voyage was, as stated in your valuable work, successful beyond expectation; and I have no doubt from the improvements made in the weapon by the patentees, Sir William Congreve, Bart. whose name stands deservedly so high in the scientific world, and Lieut. Colquhoun, since the voyage, that it will be of vital importance to those concerned in this trade in all its branches, and more particularly in the sperm fishery, since it requires little skill in its application, and wet has no effect whatever on it. I shall esteem myself particularly obliged if you will give publicity to these *facts* in any shape you may think proper; and allow me to subscribe myself, Sir,

Your most obedient humble servant,
R. C. Alderson, Lieut. Roy. Engineers.

Article XIV.

Statements of Professor Playfair respecting the University of Cambridge.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Trinity College, Oct.* 25, 1821.

I hope you will allow me to take advantage of your pages for the purpose of correcting certain mis-statements which have been recently made by some eminent writers of Scotland with respect to the history of the Newtonian philosophy in this University. The assertions of which I speak are to be found in the second part of the late Professor Playfair's "Dissertation on the History of the Mathematical and Physical Sciences," which accompanies the Supplement to the Encyclopædia Britannica: and are repeated to a certain extent in the second part of the corresponding Dissertation on the History of the Moral and Metaphysical Sciences by Mr. Dugald Stewart. The first of these authors has stated, that in the University of Cambridge the Cartesian system kept its ground for more than 30 years after the publication of Newton's discoveries in 1687: and that, at the end of that interval, the Newtonian philosophy entered the University by "stratagem," and under the protection of the Cartesian, in consequence of the publication of a translation of Rohault's Physics, accompanied with notes, by Clarke, about 1718: the purport of the notes possibly escaping the notice of the "learned doctors," who, the writer seems to have thought, had the principal direction of academical education. A belief is further expressed in a note, that "the Universities of St. Andrew's and Edinburgh were the first in Britain where the Newtonian philosophy was made the subject of the academical prelections."

I shall be as brief as possible in showing how extremely inaccurate these statements are. One of the principal proofs adduced is an expression of Whiston's, in his Memoirs, where he says that David Gregory was inculcating the Newtonian hypothesis at Edinburgh, while they ("poor wretches") at Cambridge were studying the Cartesian. Considering the great age of Whiston when he wrote his life, his expulsion from the University, and his notorious inaccuracy, he cannot be considered as unexceptionable authority on this side of the question. But it is curious enough that in the very page of his book in which this passage is found, he also speaks of setting himself "to the study of Sir Isaac Newton's wonderful discoveries, in his Philosophiæ Naturalis Principia Mathematica, one or two of which lectures," he says, "*I had heard him read in the public schools*, though I understood them not at all at that time." These "academical prelections" were probably previous to the publication of the Principia in 1687; and at all events it seems a strange undertaking to set up a claim of priority for any other lectures, in opposition to those of Newton himself upon his own philosophy. And, little as the reader would suppose it from the statements above referred to, his successors in this professorship were as zealous promulgators of his doctrines as their contemporaries in any other place. Newton was Mathematical Professor at Cambridge at the time when he published the Principia, and continued so for 16 years afterwards. The same Whiston became, in 1699, his deputy, and in 1703 his successor; in which capacities he delivered lectures, which he afterwards published (in 1707 and 1710) under the titles, "Prælectiones Astronomicæ," &c. and "Prælectiones Physico-Mathematicæ, Cantabrigiæ in Scholis Publicis habitæ, quibus Philosophia Illustrissimi Newtoni Mathematica explicatius traditur et facilius demonstratur; à Gulielmo Whiston, AM. et Matheseos Professore Lucasiano. In usum Juventutis Academicæ." In 1707 the celebrated Saunderson, having acquired an extraordinary portion of mathematical knowledge, came to Cambridge with the intention of fixing himself in the University by means of it. And though the subject was already occupied by Whiston, the blind geometer was encouraged, with the permission of the Professor himself, to give a course of lectures on "the Principia, Optics, and Arithmetica Universalis, of Newton:" which lectures, we are informed by his biographers, became extraordinarily popular. In 1711 Saunderson succeeded to the Lucasian professorship; which he held till 1739; so that I presume I may here venture to break off the chain of evidence of an uninterrupted succession, from the time of Newton himself, of professors who have delivered his philosophy from the chair which he had occupied. And so much for the claim of its priority in the academical prelections of other places.

It is further asserted that though the professors in England might, at an early period, be Newtonians, as, for instance, David Gregory, who removed from Edinburgh to Oxford in 1690; "the real and efficient system of the Universities was not cast in that mould till long afterwards." Now why we should suppose the lectures of the *scholar* at Edinburgh or St. Andrew's, to have been more efficient than the lectures either of the same person or of his *master*, at one of the English Universities, I am completely at a loss to discover. I do not, however, mean that the sublime system of our wonderful philosopher was universally adopted or understood as soon as it was delivered. I believe, that at that time the possession of the knowledge and qualifications requisite for the study of the Principia was very rare in any University; and the reception of that memorable work among the great continental geometers is a sufficient proof that it was not sure of finding favour even with men of eminent mathematical attainments, and great love of truth. It must of necessity have required some time to pervade so great a number of persons, of such various talents and tastes, as are in the English Universities thought necessary for effectual instruction. Especially too when it is considered that the subject to which the discoveries referred, formed only a part, and at that time not a prominent part, of the course of academical studies. We do, however, find very early indications of the Newtonian principles making their way into all parts of the system of the University. About 1694, the celebrated Samuel Clarke, then an undergraduate, defended in the schools a question taken from the philosophy of Newton—a step which must have had the approbation of the moderator who presided at the disputations: and his translation of Rohault, with references in the notes to the Principia, was first published in 1697; and not in 1718, as Prof. Playfair has strangely asserted It was republished in 1702 with more copious additions from the principles of Newton, which could hardly "escape the notice" of any body who saw the book, since they are mentioned in the title page.* Public exercises, or *acts* as they are called, founded on every part of the Newtonian system, are spoken of by Saunderson's biographers† as very common about 1707. By this time these studies were extensively diffused in the University; and it is mentioned that the Principia rose to above four times its original price.‡ In 1709-10, when Dr. Laughton, of Clare Hall, a zealous Newtonian, was proctor, instead of appointing a moderator, he discharged the office himself; and by the most active exertions, stimulated still further the progress of mathematical science. He had previously published a paper of questions on the Newtonian Philosophy, apparently as theses for the disputations. He had been tutor in Clare Hall from 1694. The lectures of persons in that capacity Prof. Playfair considers as the only effective part of the University system; and according to him, these instructions were very late in receiving the impression of Newtonianism. Dr. Laughton's had probably been on Newtonian principles for the whole or the greater part of his tutorship; but it is certain that for some years he had been diligently inculcating those doctrines, and that the credit and popularity of his college had risen very high in consequence of his reputation. It may be remarked also, that Cotes, the friend and disciple of Newton and Bentley, who first made his philosophy known to the readers of general literature, resided in Cambridge during the time of which we are speaking; the one as Plumian Professor, and the other as Master of Trinity College; and it can hardly be supposed that their influence would not be exerted in favour of the system which they admired. This indeed might be the less necessary, as there is not, so far as I have discovered, the slightest circumstance which indicates any opposition to its introduction.

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* A third edition appeared in 1710, with mathematical investigations, by Mr. Charles Morgan, of the laws of falling bodies, the rainbow, &c. which contained as good an elementary exposition of those parts of applied mathematics, as, I believe, existed at that time: so that the book might probably, as Prof. Playfair asserts, be in use at a later period. What misled Prof. Playfair so far as to induce him to assign 1718 as the date of Clarke's translation, I am at a loss to imagine; except it were that he took his information from Hutton's Mathematical Dictionary, under the word *Rohault*, where the edition of 1718 (the fourth) is the only one mentioned.

† See Preface to his Algebra.

‡ From 10*s*. or 12*s*. to 2*l*. 2*s*. For these particulars see Nichols' Literary Anecdotes, vol. iii. p. 332.

It is unnecessary to make any separate answer to the observations of Mr. Stewart;* as even if we allow his assertions, they will not imply any thing very disgraceful to us. They amount to this; that the philosophy of Newton was *publicly taught* at Edinburgh and St. Andrew's before it was *generally adopted* at Cambridge. That this was after it had been *publicly taught* here, I think I have proved. The Scotch were fortunate in possessing in the Gregorys men of great mathematical talents, of minds open to conviction, and of industry and capacity to master in a short time a new system of the universe; but even they, we may suppose, could not transfuse these qualifications at once into the whole body of their pupils. After what time the Newtonian doctrines had been *studied* in Scotland to the extent which the facts above mentioned indicate with respect to Cambridge, the very different constitution of their academical establishments from ours, gives us no means of judging.

Without attempting to trace further the history and progress of that philosophy which is now so zealously cultivated in the University of Cambridge, I have, I trust, sufficiently shown that the assertions with respect to the tardy influence of Newtonianism, have been hazarded with great inattention to facts; and I may be allowed to add, that it seems very doubtful whether evidence equally strong can be produced of its early prevalence in any other academical institution. The respect and admiration which are attached to the names with whose authority the assertions in question have come to us, feelings in which I sincerely participate, make it highly desirable that their inaccuracy should be exposed. In reply to misrepresentations so extraordinary, I have not allowed myself to go beyond a plain statement of facts.

I am, Sir,
Your obedient servant.

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* It would be exceedingly interesting, and might throw some light upon the question, to see a copy of the "Compend of Newton's Principia," of which mention is made in Hutton's Dictionary, and quoted by Mr. Stewart, and which is there said to have supplied Theses for academical disputations at Edinburgh in 1690. The interval between the publication of the Principia and the date of this document is extraordinarily short: the candidates for degrees who could in 1690 defend such a series of positions, must have begun to study that work the moment it issued from the press; except we suppose that then, when the ideas it contained were so new, and when the preparatory mathematics were so much more laborious than they are now, it occupied a shorter time than it is found to require from a modern student.

ARTICLE XV.

ANALYSES OF BOOKS.

Philosophical Transactions for the Year 1821. *Part II.*

(*Concluded from* p. 63.)

XVI. *Observations on Napthaline, a peculiar Substance resembling a concrete essential Oil, which is apparently produced during the Decomposition of Coal Tar by Exposure to a Red Heat.* By J. Kidd, MD. Professor of Chemistry, Oxford. (Communicated by W. H. Wollaston, MD. FRS.)

Dr. Kidd observes that although this substance has been noticed both in the *Annals of Philosophy* and the Institution Journal, there has not yet appeared, as far as he has been able to discover, any systematic description of the mode by which it may be obtained, or of its relation to the substance from which it was produced.

For the mode in which this substanee (which Dr. Kidd proposes to call naphthaline) is usually obtained, we refer to the journals already mentioned. Dr. Kidd procured it with several other products by passing the vapour of coal tar through an ignited iron tube.

He first obtained in a condensing vessel an aqueous fluid, having an ammoniacal odour, and a dark-coloured liquid, resembling tar in appearance; the properties of both of which are minutely detailed. Some of this dark-coloured liquid was submitted to slow distillation; the product consisted of two fluids, one of which had the appearance of oil, and the other of water. After these products had passed over, "a concrete substance, as white as snow, began to collect in dispersed crystalline flocculi, in the upper part of the body and neck of the retort, so as, in a short time, almost wholly to obstruct the passage." This was the naphthaline sought for, and its properties are thus given by Dr. Kidd:

"*Properties of the white concrete Substance.*—Taste, pungent and aromatic.

"It is particularly characterized by its odour, which is faintly aromatic, and not unlike that of the narcissus and some other fragrant flowers. This odour is readily diffused through the surrounding atmosphere to the distance of several feet, and obstinately adheres for a long time to any substance to which it has been communicated.

"When in its purest state, and reduced to powder, it is exceedingly smooth, and slightly unctuous to the touch; is perfectly white, and of a silvery lustre.

"Specific gravity rather greater than that of water.

"It does not very readily evaporate at the common atmospherical temperature; for, a comparison being made between this substance and camphor, in the quantity of half a grain of each in a very minute state of division, it was found that the camphor had entirely disappeared at the end of 18 hours, while the substance in question had not disappeared entirely at the end of four days.

"A quantity of it being exposed to heat in a glass vessel soon melted; but did not begin to boil till the temperature had reached 410° of Fahrenheit: the heat being then withdrawn, it remained liquid till cooled down to 180°; at which point the lowest portion was seen suddenly to congeal: the remaining portion congealed gradually; and when the whole had become solid, its temperature was 170°. The structure of the congealed mass was distinctly crystalline, and the crystalline laminæ were slightly flexible.

"It is not very readily inflamed; but when inflamed, it burns rapidly, and emits an unusually copious and dense smoke, which soon breaks into distinct particles that fall down in every direction.

"Does not affect the colour either of litmus or of turmeric.

"Insoluble in cold water; and very sparingly soluble in boiling water, from which it separates, in cooling, in such a manner as to render the water milky, which was before transparent: a portion, however, still remains dissolved, for the water, when filtered, possesses in a slight degree the taste and odour of the substance, and after a few hours deposits it in minute crystals.

"Readily soluble in alcohol, and still more so in ether, at any temperature; the solubility, in either instance, greatly increased by increase of temperature.

"A solution of this substance in four times its weight of boiling alcohol becomes, in cooling, a solid crystalline mass. It is precipitated from its solution in alcohol by water, without acquiring any additional weight.

"It is soluble in olive oil, and in oil of turpentine.

"It does not combine either with an aqueous solution of potash or ammonia; nor is it sensibly affected by contact with ammoniacal gas.

"Soluble in acetic and in oxalic acid, to each of which it communicates a clear pink colour. A saturated hot acetic solution becomes a solid crystalline mass in cooling.

"It blackens sulphuric acid when boiled in it; the addition of water to the mixture having no other effect than to dilute the colour: neither does any precipitation take place upon saturating the acid with ammonia.

"Sparingly soluble in hot muriatic acid, to which it communicates a purplish pink colour.

"When boiled in nitric acid, it both decomposes the acid, and is itself altered in its composition; and, in cooling, is abundantly deposited in short acicular crystals aggregated in stelliform groups. These crystals pressed between folds of unsized paper, in order to separate the adhering acid, and then exposed to heat, are readily melted: in cooling, the melted mass shows evident traces of acicular crystallization, and the crystals are of a yellow colour. This yellow substance is readily inflamed, burns with a bright flame, emits much smoke, and leaves a considerable residuum of carbon.

"Of all the characters of the white concrete substance described in this section, its ready disposition to crystallize is, perhaps, the most remarkable.

"If thrown into a red-hot crucible, a dense white vapour arises from it; which, being received into a bell glass placed over the crucible, is condensed round the lower part of the glass in the form of a white powder; but in the upper and cooler part of the glass distinctly crystalline plates are formed, of a beautiful silvery lustre.

"A similar and equally beautiful crystallization may be obtained by boiling this substance in water, in a glass matrass having a long neck; in the upper part of which crystals will be formed and deposited during the boiling.

"If exposed to a degree of heat not more than sufficient to melt it under a bell glass, the vapour that rises from it crystallizes before it reaches the surface of the glass, and flies about the interior with exactly the appearance of a shower of minute particles of snow.

"If a piece of cotton twine be coiled up like the wick of a candle, and after having been dipped in this substance while melted, be set on fire for a second or two, and then blown out, the vapour will soon begin to crystallize round the wick in very distinct thin transparent laminæ.

"This experiment affords one mark of distinction between this substance and benzoic acid, and also between it and camphor; for under other similar circumstances, benzoic acid crystallizes in acicular crystals, which are often grouped in a stelliform; and camphor crystallizes, or is rather congealed, in globular particles having a stalagmitic appearance.

"The most usual crystalline form of this substance is a rhombic plate, of which the greater angle appears to be from 100° to 105°: crystals at least of that form I have repeatedly obtained from its solutions in water, in alcohol, in acetic acid, in the yellow oil described in the last section; and lastly, by melting and very slowly cooling the substance itself. Sometimes several of these plates are variously grouped together; sometimes a single plate intersects another plate at nearly right angles, so that in some points of view the compound crystal appears simply cruciform. The only distinct modifications which I have observed of the common form are a rhomboidal plate, which is very nearly rectangular; and an hexagonal plate:

the latter variety may be easily traced from the rhombic plate by the incomplete developement of the smaller angles of the usual rhomb."

With respect to the elementary constitution of this substance, Dr. Kidd says, that he is "not enabled to give any satisfactory information; but it is evident that it contains a very great proportion of carbon."

It is greatly to be regreted, that Dr. Kidd did not complete his labours by giving an analysis of this curious substance.

XVII. *On the Aberrations of Compound Lenses and Object Glasses*. By J. F. W. Herschel, Esq. FRS. &c.

In the commencement of this elaborate communication, its author remarks, that it has been made a subject of reproach to mathematicians who have occupied themselves with the theory of the refracting telescope, that the practical benefit derived from their speculations has by no means been commensurate to the expenditure of analytical skill they have called for. Mr. Herschel has, therefore, in this valuable paper supplied the artist with practical matter which cannot fail to prove of the highest utility.

XVIII. *An Account of the Skeletons of the Dugong, two horned Rhinoceros, and Tapir of Sumatra, sent to England by Sir Thomas Stamford Raffles, Governor of Bencoolen.* By Sir Everard Home, Bart. VPRS.

With respect to the dugong, Sir Everard remarks, that "The bones of the skeleton, when mounted, give us a form very different from what is met with in the whale tribe. It may be compared to a boat without a keel, with the bottom uppermost; so that in the sea, the middle part of the back is the highest point in the water; and as the lungs are extended to great length on the two sides, close to the spine, they furnish the means of the animal becoming buoyant, and when no muscular exertion is made, the body will naturally float in an horizontal posture.

"When we consider that this animal is the only one yet known that grazes at the bottom of the sea (if the expression may be allowed), and is not supported on four legs, we must admit that it will require a particular mode of balancing its body over the weeds upon which it feeds.

"The hippopotamus, an animal that uses the same kind of food, from the strength of its limbs, supports itself under water; and the dugong, as a compensation for not being able to support its body on the ground, has this means of steadily suspending itself in the sea peculiar to itself, the centre of the back forming the point of suspension, similar to the fulcrum of a pair of scales. This peculiarity of position explains the form of the jaws, which are bent down at an angle with the skull, unlike the jaws of other animals. This new mode of floating, when compared with that of other sea animals, makes a beautiful variety. The balæna mysticetus, that goes to the bottom of unfathomable depths to catch in its whale-bone net the shrimps that live in that situation is surrounded by blubber not unlike a cork jacket.

"The enormous spermaceti whale, whose prey is not so far removed from the surface, has the mass of spermaceti in a bony concavity upon the skull.

"The shark tribe have the liver loaded with oil, placed in nearly the same situation as the lungs of the dugong.

"As there are no vegetables (I believe) growing at the bottom of the sea in very deep water, the nice adjustment of the body of the dugong is confined to the shallows in the creeks near the land."

Upon comparing the bones of the two-horned rhinoceros with those of the single-horned species, Sir Everard observes, that there is no difference deserving of particular remark, except that in the two-horned, the projection towards the front of the skull formed by the union of the nasal bones, is more nearly in a straight line, and more extended. This peculiarity, he thinks, may be required to give sufficient surface for two horns.

The tapir of Sumatra, as well as that of America, are stated to have a greater general resemblance to the rhinoceros than to any other animal.

This paper is accompanied by five plates; and an account of the viscera of these animals is also given; for which we refer to the original paper.

XIX. *On the Mean Density of the Earth.* By Dr. Charles Hutton, FRS.

It appears from this paper that in two instances only the mean density of the earth has been certainly or approximately determined by experiment.

"The former of these experiments," Dr. Hutton says, "was made by Dr. Maskelyne, in the years 1774, 1775, and 1776, by means of that large mountain* in Scotland, in measuring its dimensions, and in comparing its attraction on a plummet with that of the whole earth on the same; the calculations on it having been made by myself, and first published in the Philosophical Transactions of the year 1778; and since more correctly in the second volume of my Mathematical Tracts. The other experiment, by Mr. Cavendish, was by observing the attraction on small pendulous balls, of two inches diameter, by larger ones of ten inches diameter, as compared with the attraction of the earth on the same.

"By some strange mistake, or perversion, for many years, it was customary among certain persons, to withhold the mention of my name with regard to the great share that I had in the experiment on Schehallien. But from certain complaints which I have made, some little justice has lately been awarded to me on that head; though still, it would seem with reluctance, as the opinion is promptly assumed that the latter small experiment is susceptible of the greater accuracy, and the numbers in its result gratuitously adopted as nearer the truth than that of the former. As this is an opinion which I have never been able to bring my mind to acknowledge, and as it is a matter of great importance in the present state of physics, I have been desirous to draw the attention of philosophers to a closer consideration of the subject, with a view to a more deliberate and impartial decision of this point.

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* Schehallian.

"From the closest and most scrupulous attention I can employ on this question, the preference, in point of accuracy, appears to be decidedly in favour of the large or mountain experiment over that of the small balls."

From the Schehallian experiment, Dr. H. thinks, that the mean density of the earth is five times that of water, but not more; while, from his experiment, Mr. Cavendish has assumed the mean density to be = 5·48. Dr. Hutton afterwards points out some of the errata in Mr. Cavendish's paper; and concludes that the medium of the first six of his experiments is 5·19, and of the other 23 experiments 5·43, and the mean of both these means, he observes, is 5·31, instead of 5·48, the error arising from the sum of the numerical calculations. The difference of 0·31, or about the 17th part of the whole, Dr. Hutton thinks must be ascribed to the inaccuracy of making and reading off experiments with such intricate and inadequate machinery as that used by Mr. Cavendish.

Dr. Hutton observes, "that he cannot conclude this paper of inquiry without expressing a hearty wish for the repetition of the large or mountain experiment, in some other favourable situation, and with improved means, if possible." For this purpose, the Doctor suggests an idea, that one of the large pyramids in Egypt might profitably be employed, instead of a mountain, for this experiment.

XX. *On the Separation of Iron from other Metals.* By J. F. W. Herschell, Esq. FRS. &c.

This paper is given in the present number of the *Annals.*

XXI. *On the Re-establishment of a Canal in the Place of a Portion of the Urethra which had been destroyed.* By Henry Earle, Esq. Surgeon to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital. (Communicated by Sir Humphry Davy, Bart. PRS.)

For the particulars of this paper, which is entirely surgical, we must refer to the original volume.

XXII. *Calculations of some Observations of the Solar Eclipse on Sept.* 7, 1820. By Mr. Charles Rumker, (Communicated by Thomas Young, MD. For. Sec. to RS.)

For this paper, which can hardly be abridged, we must also refer to the volume.

XXIII. *An Account of the Remeasurement of the Cube, Cylin-*

der, and Sphere, used by the late Sir George Shuckburgh Evelyn, in his Inquiries respecting a Standard of Weights and Measures. By Capt. Henry Kater, FRS.

The experiments above referred to are detailed in the Philosophical Transactions for 1798; and "it may there be seen," says Capt. K. "that a cube, a cylinder, and a sphere of brass, were employed, the respective dimensions of which being given, as well as the weight of water displaced by each, the weight of a cubic inch of distilled water might thence be readily ascertained.

"In reviewing these experiments, so much care appears to have been bestowed on those parts of the inquiry which relate to *weight*, as to leave no reason to doubt their accuracy; but as Sir George Shuckburgh has not entered into so full a detail of the method he pursued in the *measurement* of the cube, the cylinder, and the sphere, I felt it to be desirable that this operation should be repeated before the Commissioners of Weights and Measures should make their final Report."

Sir George's experiments were repeated by Captain Kater, and the paper concludes by collecting under one view the data furnished by Sir George Shuckburgh's experiments and his own measurements, and he observes, that "From these data, the weight of a cubic inch of distilled water in a vacuum at 62°, deduced from the cube, appears to be

	252·907 of Sir G. Shuckburgh's grains.
From the cylinder.....	252·851
From the sphere	252·907
The mean of which is..	252·838

which is equal to 252·722 grains of the Parliamentary Standard."

XXIV. *An Account of Observations made with the Eight Feet Astronomical Circle, at the Observatory of Trinity College, Dublin, since the Beginning of the Year* 1818, *for investigating the Effects of Parallax and Aberration on the Places of certain Fixed Stars; also the Comparison of these with former Observations for determining the Effects of Lunar Nutation.* By the Rev. John Brinkley, DD. FRS. and MRIA. Andrews Professor of Astronomy in the University of Dublin.

Respecting this very elaborate paper, we can scarcely say any more than state the purposes for which the experiments detailed in it were undertaken, which are thus mentioned by Dr. B.

"The results of the observations which I now beg leave to lay before the Royal Society, were instituted with a view of discovering, if possible, the source of the differences that has existed between the results of former observations made here, and of others made at the Royal Observatory at Greenwich; and they will, it is imagined, be found to be useful relative to some other important points in astronomy.

"My former observations of certain stars pointed out a deviation of about one second from the mean place, after having made all the usual corrections. Mr. Pond's observations pointed out no such deviations. The deviations that I had found agreed with the effects of parallax. The observations that I have since made, far more numerous than the former, concur in exhibiting the same results: in showing deviations in certain stars that can be explained by parallax. Every other suggested solution of the difficulty appears quite inadequate thereto. It is, I think, nearly demonstrated, that no change of figure in the instrument has occasioned it, and that the uncertainties of the changes of refraction can have had only a very small share, if any, in producing the effect observed."

XXV. *On the Effects produced in the Rates of Chronometers by the Proximity of Masses of Iron.* By Peter Barlow, Esq. of the Royal Military Academy. (Communicated by John Barrow, Esq. FRS.)

Mr. Fisher, who accompanied Capt. Buchan in 1818 to the Arctic Regions, has shown, in a paper printed in the Philosophical Transactions for 1820, that the rates of chronometers differ on shore and on board, losing in the former situation, but gaining in the latter: this variation he ascribed to the influence of the ship's iron on the balance, and his communication contains a detail of the various experiments which he performed, to show that the magnetic influence tends to accelerate the motion of the time-keepers by its influence on the steel part of their balances. Referring to Mr. Fisher's experiments, Mr. Barlow found that five out of the six chronometers which he used were retarded, while all Mr. Fisher's were accelerated.

From a number of experiments which have been made, Mr. Barlow says, it appears "that a chronometer ought to be kept as carefully at a distance from any partial mass of iron, as the compass itself."

XXVI. *On the Peculiarities that distinguish the Manatee of the West Indies from the Dugong of the East Indian Seas.* By Sir Everard Home, Bart. VPRS.

XXVII. *On a new Compound of Chlorine and Carbon.* By Richard Phillips, FRSE. FLS. MGS. &c. and Michael Faraday, Chemical Assistant in the Royal Institution. (Communicated by Sir Humphry Davy, Bart. PRS.)

We have already given some account of this paper in the *Annals.*

XXVIII. *On the Nerves; giving an Account of some Experiments on their Structure and Functions, which lead to a new Arrangement of the System.* By Charles Bell, Esq. (Communicated by Sir Humphry Davy, Bart. PRS.)

XXIX. *Further Researches on the Magnetic Phænomena produced by Electricity; with some new Experiments on the Properties of Electrified Bodies in their Relations to conducting Powers and Temperature.* By Sir Humphry Davy, Bart. PRS.

This paper was printed entire in the last number of the *Annals.*

ARTICLE XVI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The following papers have been read since our last report:

Jan. 10.—A letter from Capt. Hall, containing Observations on a Comet seen at Valparaiso.

Elements of the above Comet, by Dr. Brinkley.

Jan. 17.—On ultimate Atoms of the Atmosphere, by Dr. Wollaston.

On the Expansion in a Series of the Attraction of a Spheroid, by James Ivory, Esq.

Jan. 24.—On the late Depression of the Barometer, by Luke Howard, Esq.

On the anomalous Magnetic Attraction of Hot Iron, by P. Barlow, Esq.

ARTICLE XVII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Preparation of Quinine.*

M. J. Voreton, of Grenoble, employs the following method in preparing Quinine, by which he says he is enabled to procure about two ounces and a half of Quinine from eleven pounds of Cinchona, instead of an ounce and a half, or an ounce and three quarters procured by the common process. The Cinchona reduced to a coarse powder is to be digested in water, acidulated with about one hundredth of its weight of muriatic acid. At the expiration of 24 hours, the Cinchona is to be strongly pressed, to be again treated with dilute muriatic acid, and the processes are to be repeated, till the Cinchona loses its bitterness. The filtered infusions are to be mixed and treated with excess of pure magnesia, the mixture to be boiled for a short time and then suffered to cool. The magnesian precipitate is to be washed with cold water, dried, and digested in alcohol: by distilling this solution the Quinine is obtained.—(Annales de Chimie.)

II. *Improvement in Stringed Instruments.*

M. Fischer of Leipsic, recommends the use of platinum wire, as a substitute for brass and steel, in the strings of musical instruments. He describes the tone as much finer than in instruments in which the usual metals are employed, and it certainly posseses the almost unique advantage of preserving its metallic brillancy, though surrounded by the continued deposit of a damp atmosphere.—(Rev. Ency. Oct. 1821.)

III. *Comparative Analysis of the Food and Excrement of the Nightingale.*

M. Braconnot having collected the excrement of a nightingale, with the intention of extracting uric acid from it, which it contains in great abundance, afterwards undertook to compare its constituent principles with those of the ox's heart upon which the bird was fed.

Three hundred parts of ox's heart yielded the following substances:

Water	231·11
Fibrine, vessels, nerves, cellular membrane, fat, and phosphate of lime	54·59
Albumen retaining the colouring matter of the blood, phosphate of lime and magnesia	8·20
Extractive matter soluble in alcohol (ozmasome)	4·70
Lactate of potash	0·56
Phosphate of potash	0·46
Chloride of potassium	0·38
Ammoniacal salt and free acid	A trace
	300·00

Thirty-six parts of the excrement of the nightingale yielded the following substances:

Super-urate of potash and ammonia	19·00
A peculiar substance slightly animalized, soluble in water, and insoluble in alcohol	12·00
Ferruginous phosphate of lime	1·50
Sulphate of potash	1·20
Picromel	1·00
Chloride of potassium	0·23
Phosphate of potash and ammonia	0·23
Unknown combustible acid combined with ammonia	0·20
Ammoniaco-magnesian phosphate	0·08
Free lactic and acetic acid about	0·10
Mucus	0·10
Peculiar black matter, resembling that found in urine by M. Proust, from 1 to	0·10
A brown thick oil readily soluble in alkalies and in alcohol from 0·5 to	0·05
Muriate of ammonia estimated at	0·05
	35·84

(Annales de Chimie.)

IV. *Analysis of Black and Green Tea.*

Mr. Brande has lately made a comparative analysis of black and green tea, from which he finds that "the quantity of astringent matter precipitable by gelatine is somewhat greater in green than in black tea, though the excess is by no means so great as the comparative flavours of the two would lead one to expect. It also appears that the entire quantity of soluble matter is greater in green than in black tea;

and that the proportion of extractive matter not precipitable by gelatine is greatest in the latter."

"Sulphuric, muriatic, and acetic acids, but especially the first, occasion precipitates in infusions both of black and green tea, which have the properties of combinations of those acids with tan. Both infusions also yield, as might be expected, abundant black precipitates, with solutions of iron; and when mixed with acetate, or more especially with subacetate of lead, a bulky buff-coloured matter is separated, leaving the remaining fluid entirely tasteless and colourless. This precipitate was diffused through water, and decomposed by sulphuretted hydrogen; it afforded a solution of tan and extract, but not any traces of any peculiar principle to which certain medical effects of tea, especially of green tea, could be attributed."

Mr. Brande observes, that there is one property of strong infusions of tea, belonging especially to black and green, which seems to announce the presence of a distinct vegetable principle; namely, that they deposit, as they cool, a brown pulverulent precipitate, which passes through ordinary filters, and can only be collected by deposition and decantation; this precipitate is very slightly soluble in cold water of the temperature of from 50° downwards, but it dissolves with the utmost facility in water of 100° and upwards, forming a pale-brown transparent liquid, which furnished abundant precipitate in solutions of isinglass, of sulphate of iron, of muriate of tin, and of acetate of lead; whence it may be inferred to consist of tannin, gallic acid, and extractive matter.

The following table is given by Mr. Brande as showing the respective quantities of soluble matter in water and alcohol, the weight of the precipitate by isinglass, and the proportion of inert woody fibre on green and black tea of various prices:

One hundred parts of tea.	Soluble in water.	Soluble in alcohol.	Precipitate with jelly.	Inert residue.
Green hyson, 14*s.* per lb. ..	41	44	31	56
Ditto, 12*s.*	34	43	29	57
Ditto, 10*s.*	36	43	26	57
Ditto, 8*s.*	36	42	25	58
Ditto, 7*s.*	31	41	24	59
Black souchong, 12*s.*	35	36	28	64
Ditto, 10*s.*	34	37	28	63
Ditto, 7*s.*	36	35	24	64
Ditto, 6*s.*	35	31	23	65

(Royal Institution Journal.)

V. *Spontaneous Explosion of Chlorine and Hydrogen.*

It has been long known that a mixture of chlorine and hydrogen explodes when exposed to the direct action of the sun's rays. In order to try if this effect could be produced by the radiation of a common culinary fire, Professor Silliman filled a common Florence oil-flask (well cleaned) half full of chlorine gas, and was in the act of introducing the hydrogen in the pneumatic cistern. "There was not only no *direct* emanation from the sun, but even the *diffuse* light was rendered much feebler than common by a thick snow-storm, which had covered the skylight above with a thick mantle, and veiled the heavens in a singular degree for such a storm. Under these circumstances, the hydrogen was scarcely all introduced before the flask exploded with a distinct flame; portions of the glass stuck in the woodwork of the ceiling of the room, and the face and eyes escaped by being out of the direction of the explosion; nothing but the neck of the flask remained in hand. This occurrence then proves, that a mixture of chlorine and hydrogen gas may explode spontaneously in a diffuse light, and even in a very dim light."—(American Journal of Science, Vol. 3, No. 2, p. 343.)

VI. *Sulphato-tricarbonate of Lead.*

A very fine specimen of carbonate of lead was recently brought from Leadhills, by Alexander Irving, Esq. who found it by analysis to be a sulphato-carbonate. Upon examining its crystals, I find it to be the sulphato-tricarbonate of Mr. Brooke. The crystals, which are of considerable size, are acute rhomboids, with cleavages perpendicular to the axis of the rhomb. They are of a bright sap-green colour. Upon examining their optical structure, I find that they have two axes of double refraction, the principal one of which is coincident with the axis of the rhomb. The sulphato-tricarbonate, therefore, cannot have the acute rhomboid for its primitive form, but must belong to the prismatic system of Mohs. D. B.—Edin. Phil. Jour.

VII. *Calc-sinter determined to be true Calcareous Spar.*

The Rev. Dr. Fleming, of Flisk, transmitted to me lately two specimens of this substance, with the following remark: "Lamellar calc-sinter from Macalister's Cave in Sky. I procured these crystals in shallow pools in the cave filled with the calcareous water. The indications of crystallization are distinct, but the crystals seem to be but in progress. The summits of the crystals of the smallest piece are smooth and flat, and indicate the prisms below to be five-sided, and sometimes four-sided. I regard these specimens as exceedingly curious, as they are genuine examples of Neptunian calcareous spar. 2. Acicularly Crystallized fibrous Calc-sinter.—This substance is from the Isle of Man; the specimen from which these fragments were separated was given me by Mr. Stevenson several years ago, and is interesting as being a recent aqueous formation." Dr. Fleming adds, "that all the calcareous matter in Macalister's Cave, whatever be its external form, stalactitic, stalagmitic, or encrusting, is all more or less in the state of calcareous spar, with the usually foliated structure: that which lies in the pools or hollows of the caves has its crystalline forms like those in the specimens sent." Upon examining these interesting specimens, I succeeded in extracting from them regular rhombs of calcareous spar, having their angles of the same value as the finest specimens of carbonate of lime. Their double refraction and their polarising force, were of the same character and the same intensity as the purest Iceland spar. D. B.—(Edin. Phil. Jour.)

VIII. *New Mineral from Aachen, near Altenberg.*

Having examined a very fine crystal of Stilbite from Aachen, near

Altenberg, which Mr. Heuland was so kind as to transmit to me, I have found it to differ essentially from all the stilbites, and even from the new species into which Mr. Brooke has separated the substances formerly ranked under this name. Since I examined this mineral, I have learned that it is considered by Hauy as a variety of stilbite, to which he gives the name of Duo-vigesimale. D. B.—(Edin. Phil. Jour.)

IX. *On the Spurs of the Ornithorynchus.*

Dr. Traill, of Liverpool, has lately had an opportunity of examining the skins of a male and female ornithorynchus from New South Wales. The spurs of the male were remarkably strong and sharp, and the perforation in them so extremely minute, that it is not surprising that they escaped the notice of the first naturalists who examined them. The tubes were so fine that they would not receive a horse hair, though they admitted a human one.—(Edin. Phil. Jour.)

X. *Methods of kindling Fire on the Sandwich Islands.*

There are various methods of producing fire. In the Caroline Islands, a piece of wood being held fast on the ground, another short piece, about a foot and a half long, of the thickness of a thumb, even, as if turned, and with the end bluntly rounded off, is held perpendicularly over it, and put in motion between the palm of the hand, like the mill used for making chocolate. The motion is at first slow, but is accumulated, and the pressure increased, when the dust produced by the friction collects round the bores, and begins to be ignited. This dust is the tinder which takes fire. The women of Eap are said to be uncommonly clever at this process. In Radack and the Sandwich Islands, they hold on the under piece of wood another piece a span long, with a blunt point, at an angle of about 30 degrees, the point of the angle being turned from the person employed. They hold the piece of wood with both hands, the thumbs below, the fingers above, so that it may press firmly and equally, and thus move it backwards and forwards in a straight line, about two or three inches long. When the dust that collects in the groove, produced by the point of the stick, begins to be heated, the pressure and the rapidity of the motion are increased. It is to be observed, that in both methods two pieces of the same kind of wood are used; for which purpose, some of equally fine grains, not too hard, and not too soft, are the best. Both methods require practice, dexterity, and patience. The process of the Aleutians is the first of these methods, improved by mechanism. They manage the upright stick in the same manner as the gimlet or borer which they employ in their work. They hold and draw the string, which is twice wound round it with both hands, the upper end turning in a piece of wood, which they hold with their mouth. In this way, I have seen a piece of fir turned on another piece of fir, produce fire in a few seconds; whereas, in general, a much longer time is required. The Aleutians also make fire by taking two stones with sulphur rubbed on them, which they strike together over dry moss strewed with sulphur.—(Kotzebue's Voyage, iii. 259.)

XI. *Method of illuminating the Dials of Public Clocks with Gas.*

Messrs. John and Robert Hart, of Glasgow, who have been long known to the public for their scientific acquirements, as well as their practical ingenuity, have erected a very ingenious apparatus for illuminating with gas the dials of the Tron Church and Post-office steeple in Glasgow. "The apparatus consists of a No. 1 Argand burner, placed a few feet out from the top of the dial, and enclosed in a nearly hemispherical lantern, the front of which is glazed; the back forms a parabolic reflector; the dial receives not only the direct, but a conical stream of reflected rays, and is thus so brilliantly illuminated, that the hours and hands can be seen with nearly the same distinctness at a distance as through the day. To mask the obtuse appearance of the lantern, its back has been made to assume the form of a spread eagle, above which is placed the city arms, the whole handsomely executed and gilt. The gas-pipe and lantern move on an air-tight joint, so that the lantern may be brought close to the steeple for cleaning when necessary. The gas is first ignited by means of a train or flash-pipe, so perforated, that when the gas issuing from the holes at the one end is lighted, the holes along the pipe become so, and thus the gas inside the lantern is kindled as if by a train of dry gunpowder: in this way the light might be first communicated either from the street or from the steeple. The effect of the lighted dial is at once cheerful, pleasant, and useful. By a simple contrivance, the clock disengages a small detent, something similar to the larum in wooden clocks. This shuts the gas cock, and instantly extinguishes the light."—(Edin. Philos. Journal.)

ARTICLE XVIII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Mr. Children has in the press a Translation of Professor Berzelius's work on the Use of the Blowpipe in Chemical Analyses and Mineralogical Investigations, with Notes and other Additions by himself. It will form an octavo volume, and be illustrated with engravings.

A comparative Estimate of the Mineral and Mosaical Geologies. By Greaville Penn, Esq. 1 vol. 8vo.

JUST PUBLISHED.

A Description of the Shetland Islands, comprising an Account of their Geology, Scenery, Antiquities, and Superstitions. By Samuel Hibbert, MD. MPSE. 4to. With Maps and Plates, 3*l*. 3*s*.

Twelve Essays on the Proximate Causes of the Aggregate and Atomic Phænomena of the Universe, Physical, Mechanical, Chemical, and Organical. By Sir Richard Phillips. 8vo. With Plates. 9*s*.

A Treatise on Diseases of the Nervous System. Vol. I. on Convulsive and Maniacal Affections. By J. C. Prichard, MD. &c. 8vo. 12*s*.

A Treatise on Diseases of the Chest. Translated from the French of R. T. H. Laennec, MD. By John Forbes, MD. 8vo. 14*s*.

The Encyclopædia Metropolitana, or Universal Dictionary of Knowledge, on an Original Plan, comprising the two-fold Advantages of a Philosophical and Alphabetical Arrangement. With appropriate and entirely new Engravings. Part V. 1*l.* 1*s.* To be continued regularly till completed.

The Introductory Lecture of a Course upon State Medicine, delivered in Mr. Granger's Theatre, Southwark, Nov. 1, 1821. By John Elliotson, MD. &c. 8vo. 2*s.* 6*d.*

An Inquiry into the Opinions Ancient and Modern concerning Life and Organization. By John Barclay, MD. Lecturer on Anatomy and Surgery, Fellow of the Royal College of Physicians, &c. 8vo. 14*s.*

The Principles of Medicine on the Plan of the Baconian Philosophy. Vol. I. on Febrile and Inflammatory Diseases. By R. D. Hamilton. 8vo. 9*s.*

The Botanical Cultivator; or a Practical Treatise on propagating, rearing, and preserving, all Descriptions of Plants cultivated in the Hothouses, Greenhouses, and Gardens, of Great Britain. By Robert Sweet, FLS. 1 vol. 10*s.* 6*d.*

A Treatise on Bulbous Roots, particularly those heretofore included under the Genera Amaryllis, Cyrtanthus, and Pancratium. By the Hon. and Rev. William Herbert. With coloured Plates. 5*s.*

A Monography on the Genus Camellia. By Samuel Curtis, FLS. Large folio. 3*l.* 3*s.* plain. 6*l.* 16*s.* 6*d.* coloured.

Physiological Lectures. By John Abernethy. Complete in 1 vol. 8vo. 18*s.*

Essays on Surgery and Midwifery, with Practical Observations and Latent Cases. By James Barlow. 8vo. 12*s.*

History of Cultivated Vegetables. By H. Phillips. 2 vols. royal 8vo. 1*l.* 11*s.* 6*d.*

Hortus Suburbanus Londinensis; or a Catalogue of Plants, cultivated in the Neighbourhood of London; arranged according to the Linnean System: with the Addition of the Natural Orders to which they belong.

ARTICLE XIX.

NEW PATENTS.

Owen Griffith, of Tryfan, Carnarvonshire, Gent.; for an improvement in the principle and construction of manufacturing or making trusses for the cure of ruptures or hernia, in whatsoever part or parts of the body it may be situated.—Oct. 18, 1821.

Thomas Martin and Charles Grafton, of Birmingham, printing-ink manufacturers, for a method of making fine light black, of a very superior colour, which they call spirit black; and a new apparatus for producing the same.—Oct. 24.

Benjamin Thompson, of Ayton Cottage, Durham, Gent. for a method of facilitating the conveyance of carriages along iron and wood rail-ways, tram-ways, and other roads.—Oct. 24.

Charles Tuckley, sen. of Kenton-street, Brunswick-square, cabinet-maker, for certain improvements applicable to window-sashes, either single or double hung, fixed or sliding sashes, casements, window shutters, and window blinds.—Nov. 1.

Samuel Hobday, of Birmingham, patent snuffer-maker, for a method of manufacturing the furniture for umbrellas and parasols, and of uniting the same together.—Nov. 1.

John Frederick Archbold, Esq. of Serjeant's-inn, Fleet-street, London, for a mode of ventilating close carriages.—Nov. 1.

Richard Wright, of Mount-row, Kent-road, Surry, engineer, for improvements in the process of distillation.—Nov. 9.

David Redmund, of Agnes-Circus, Old-street-road, Middlesex, engineer, for an improvement in the construction or manufacture of hinges for doors.—Nov. 9.

Franz Areton Egells, of Britannia-terrace, City-road, Middlesex, engineer, for certain improvements on steam-engines.—Nov. 9.

William Westley Richards, of Birmingham, gun-maker, for an improvement in the construction of gun and pistol locks.—Nov. 10.

James Gardner, of Banbury, Oxfordshire, ironmonger, for a machine preparatory to melting in the manufacture of tallow, soap, and candles; and which machine may be used for other similar purposes.—Nov. 9.

John Bates, of Bradford, Yorkshire, machine-maker, for certain machinery for the purpose of feeding furnaces of every description, steam-engines, and other boilers, with coal, coke, and fuel of every kind.—Nov. 9.

William Penrose, of Stummorgangs, Yorkshire, miller, for various improvements in the machinery for propelling vessels, and in vessels so propelled.—Nov. 10.

Bowles Symes, of Lincoln's-inn, Esq. for an expanding hydrostatic piston, to resist the pressure of certain fluids, and slide easily in an imperfect cylinder.—Nov. 10.

Joseph Grout, of Gutter-lane, Cheapside, London, crape manufacturer, for a new manufacture of crape.—Nov. 13.

Neil Arnott, of Bedford-square, MD. for improvements connected with the production and agency of heat in furnaces, steam and air engines, distilling, evaporating, and brewing apparatus.—Nov. 14.

Richard Macnamara, Esq. of Canterbury-buildings, Lambeth, for an improvement in paving, pitching, and covering streets, roads, and other places.—Nov. 20.

John Collinge, of Lambeth, engineer, for an improvement in hinges. —Nov. 22.

Henry Robinson Palmer, of Hackney, civil engineer, for improvements in the construction of rail-ways, and tram-roads, and of the carriages to be used thereon.—Nov. 22.

Thomas Parkin, of Skinner-street, Bishopsgate-street, merchant, for an improvement in printing.—Nov. 24.

William Baylis, jun. of Painswick, Gloucestershire, clothier, for a machine for washing and cleansing clothes,—Nov. 27.

Thomas Motley, of the Strand, patent letter-maker and brass-founder; for certain improvements in the construction of candlesticks or lamps, and in candles to be burnt therein.—Nov. 27.

Robert Bill, Esq. of Newman-street, Marylebone, for an improvement in the construction of certain descriptions of boats and barges.—Dec. 5.

ARTICLE XX.

METEOROLOGICAL TABLE.

1821.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Hygr. at 9 a.m.	
		Max.	Min.	Max.	Min.				
12th Mon.									
Dec. 1	W	29·96	29·67	47	39	—	03		
2	W	29·97	29·81	51	42	—	—		
3	S W	30 00	29·75	54	33	—	80		
4	N W	30·00	29·85	48	39	—	15		
5	W	30·25	29·85	51	32	—	09		☾
6	N W	30·28	30·15	41	32	—			
7	S E	30·06	29·91	49	40	—	—		
8	W	30·16	30·06	52	46	—	—		
9	S W	30·16	30·11	52	50	—	02		
10	S	30·28	30·05	55	35	—	03		
11	N W	30·36	30·28	44	27	—			☉
12	S E	30·34	30·04	51	42	—			
13	S E	30·08	30·04	52	38	50	—		
14	S	30·04	30·04	51	41	—	03		
15	S E	30·04	29·87	51	42	—			
16	S W	29 87	29·62	54	48	—	—		
17	S W	29·62	29·24	52	42	—	48		
18	S W	29·24	29·22	52	42	—	11		☽
19	S W	29·45	29·22	50	34	—	06		
20	S W	29·45	28·50	49	38	—	31		
21	N W	29·37	28·96	48	39	—	03		
22	S W	29·51	28 60	50	41	—	26		
23	W	29·17	29·00	46	39	56	08		
24	S	29·00	27·83	48	38	—	78		
25	N W	28·43	28·21	41	28	—			
26	E	29·05	28·23	44	30	—	35		●
27	S W	29·05	28·88	46	39	—	35		
28	S E	28·46	28·40	47	36	—	68		
29	N W	29·11	28·46	46	41	—	18		
30	S E	29·90	29·11	46	34	—	03		
31	N W	29·90	29·88	41	32	25			
		30·36	27·83	55	27	1·31	4·85		

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Twelfth Month.—1. Rainy. 2. Cloudy. 3. Rainy. 4. Fine morning: rainy night. 5. Showers: some thunder in the afternoon. 6. Fine: cold. 7. Fine morning: drizzly afternoon. 8, 9, 10. Fine. 11, 12. Cloudy. 13, 14, 15. Fine. 16. Fine day: rainy night. 17. Fine morning: rain in the afternoon: night squally. 18, 19. Fine. 20. Heavy showers at intervals during the day: night stormy, with lightning. 21. Small rain in the morning: afternoon, fine. 22, 23. Fine. 24. Rainy. 25. Very fine. 26. Rainy. 27. Rainy: hail about noon. 28. Rainy: some sleet about half-past one, p. m.

⁎ This month is remarkable for a depression of the barometer, which, for London at least, or its vicinity, is nearly without a precedent on record. The lowest observation here given, 27·83 in. was obtained at Tottenham from a portable barometer of Sir H. Englefield's construction, about five, a. m. on the 25th. The barometer at the laboratory was not observed when at its lowest point. The indexes of many wheel barometers retrograded on this occasion into the *set fair* part of the scale, and were found in the vicinity of 31 inches, a circumstance which occasioned some curious remarks on the supposed inconsistency of the weather glass with the weather. We had no storm of wind of any consequence after this great depression, which, it should be remarked, had been coming on for about two weeks. It appears by the papers, that a like state of the barometer was extensively observed at the same time on the Continent, and that very tempestuous weather attended it, far to the south of our island.

RESULTS.

Winds: E, 1; SE, 6; S, 3; SW, 9; W, 5; NW 7.

Barometer: Mean height	
For the month	29·538 inches.
For the lunar period, ending the 16th	29·916
For 15 days, ending the 3d (moon south)	29·786
For 12 days, ending the 15th (moon north)	30·085
For 15 days, ending the 30th (moon south)	29·005
Thermometer: Mean height	
For the month	43·354°
For the lunar period	45·733
For 30 days, the sun in Sagittarius	45·316
Evaporation	1·31 in.
Rain	4·85

Laboratory, Stratford, First Month, 22, 1822.

R. HOWARD.

ANNALS

OF

PHILOSOPHY.

MARCH, 1822.

ARTICLE I.

Experiments to determine the Weight of an Atom of Alumina. By Thomas Thomson, MD. FRS. Regius Professor of Chemistry in the University of Glasgow.

It is not unlikely that the labour which I have bestowed in order to render the following experiments as accurate as possible will appear to some persons a waste of time. But I am of opinion that it is of the greatest importance to determine the atomic weights of bodies with the utmost possible precision. When this desirable object is gained, the art of analysis, at present so laborious and so uncertain, will be greatly simplified. Besides, alumina being a constituent so generally found in crystallized minerals, an exact knowledge of its atomic weight cannot but throw considerable light upon the constitution of a very numerous and interesting series of crystallized minerals.

I have examined a considerable number of the salts of alumina, but found none of them fit for my purpose, except common alum—a salt which crystallizes with great ease and regularity, and which can be readily obtained in a state of the most perfect purity. It is not sensibly altered by exposure to the air, and may, therefore, be obtained without difficulty at all times in the same state. It is well known, that alum consists of four different constituents, which are always present in it in exactly the same proportion; namely, sulphuric acid, alumina, potash, and water. The object of my experiments was to determine the weight of each of these constituents in 100 grains of alum crystals.

1. I may observe, before detailing my own experiments, that

we have a good many analyses of alum. But the one which appears to have been made with the greatest care, and which approaches nearest the truth, is that of Berzelius, first given to the public in the Annales de Chimie, vol. lxxxii. p. 258. The result of this analysis is as follows:

Sulphuric acid	34·23
Alumina	10·86
Potash	9·81
Water	45·00
	99·90

Upon this analysis, it may be requisite to make a few remarks.

(1.) His mode of determining the sulphuric acid was to dissolve 100 grains of alum in water, and precipitate the sulphuric acid by means of muriate of barytes. The sulphate of barytes obtained weighed exactly 99·765 grains. Now as sulphate of barytes is composed of 5 sulphuric acid + 9·75 barytes, it is obvious that 99·765 grains of the salt contain only 33·82 grains of sulphuric acid instead of 34·23 grains—the quantity stated by Berzelius; so that Berzelius overrates the acid, as found by his experiment, by about two-fifths of a grain. I shall show hereafter that the real quantity of sulphuric acid in 100 grains of alum is 32·854 grains, or nearly one grain less than the quantity indicated by Berzelius's experiment.

(2.) The alumina was obtained by dissolving 100 grains of alum in water, and precipitating it by ammonia in considerable excess. The precipitate was washed and dried in a strong red heat. The alumina thus obtained in one experiment weighed 10·86 grains; in another 10·67 grains. This is very nearly the mode which I employed. I would remark only that an excess of ammonia is not necessary. If you add simply the quantity required to saturate the sulphuric acid united with the alumina, the whole alumina will be precipitated. The advantage of this method is, that little or no alumina will be dissolved by the excess of ammonia. Berzelius indeed recovered this portion by evaporating the ammoniacal liquid to dryness; but the alumina in this case is apt to be carried off with the liquid as it evaporates. This I suspect to be the reason of the small deficiency of alumina in Berzelius's experiments. This deficiency in one case was about one-fifth grain; and in the other, two-fifths of a grain. These quantities are indeed very small; but they have a sensible effect in altering the atomic weight of alumina; for even the smallest of them amounts to nearly two per cent. of the whole weight of the alumina.

(3.) His mode of obtaining the potash was to digest 100 parts of alum in a phial with carbonate of strontian and water till the whole sulphate of alumina was decomposed and precipitated. The filtered liquid was evaporated to dryness in a platinum crucible. The sulphate of potash weighed 18·3 parts; but was found, when dissolved in water, to contain 0·15 of sulphate of strontian; so that the real quantity of sulphate of potash obtained was 18·15 parts. Now sulphate of potash is a compound of five parts acid + six parts potash; so that the true quantity of potash in 18·15 parts is 9·9 instead of 9·81, as stated by Berzelius.

(4.) Berzelius's mode of determining the water of crystallization in alum was to heat the salt in a platinum crucible over a spirit of wine lamp. The loss of weight sustained was always 45 per cent. I find that by this method we cannot drive off the whole of the water from alum. A small portion still remains which cannot be dissipated, except by the application of a red heat. The analysis of alum by Berzelius then, when corrected, gives us the following results:

Acid	33·82
Alumina	10·86
Potash	9·90
Water	45·00
	99·58

leaving a deficiency of almost half a per cent. which, as we shall see afterwards, was owing to water not driven off by the heat of a spirit of wine lamp.

II. I shall now relate as concisely as possible the experiments which I made in order to ascertain the constituents of alum; omitting, as is my usual practice, all the trials which were either unsuccessful or not more successful than those which I state. On the present occasion, the experiments which I omit were at least 10 times more numerous than those which I give; for I employed a great variety of ways to analyze alum, partly to check my results by one another, and partly to determine which mode of analysis was easiest and most to be depended on.

1. *Sulphuric Acid.*—When 60·875 grains of pure alum crystals are dissolved in water, and the solution mixed with a solution of 53 grains of chloride of barium, a white precipitate falls, consisting of sulphate of barytes. After this precipitate has subsided, if we test the clear liquid which swims over it by means of solutions of glauber salt and muriate of barytes, we shall find that it contains no traces either of barytes or of sulphuric acid. Consequently the barytes from 53 grains of chloride of barium exactly saturates the sulphuric acid in 60·875 grains of alum. But the barytes from 53 grains of chloride of barium amounts to exactly 39 grains, or 9·75 × 4; but 9·75 barytes just saturate 5 sulphuric acid. It is obvious from this, that 39 grains of barytes will just saturate 20 grains of sulphuric acid; consequently 60·875 grains of alum contain exactly 20

grains of sulphuric acid: 100 grains of alum must of course contain 32·8542 grains of sulphuric acid.

In this determination, from the great insolubility of sulphate of barytes, I believe the number to be correct at least to as many decimal places as I have given. Now Berzelius's number (even when corrected) exceeds this by 0·9658 grain.

2. *Water.*—To determine the weight of water in alum by direct experiment is attended with difficulties, which, for some time, I found insurmountable. Indeed after a vast number of attempts, I found such discordance between my results, that I was induced to suspect that the water in alum was not a constant quantity, and that of consequence an analysis of this salt perfectly correct was impossible. By degrees, however, I began to suspect the sources of the variance, and a closer inspection put me on a way of obtaining the water, if not by one simple process, yet by uniting two together. I shall give in the first place one or two of my early results, that the reader may perceive the want of agreement between them.

(1.) 100 grains of alum exposed for three hours to a heat of 600° lost 44·04 grains; or almost a grain less than Berzelius disengaged by means of a spirit lamp. We see from this that Berzelius had applied a heat exceeding 600° in intensity.

(2.) 100 grains of alum exposed to a low red heat lost 70·72 grains. This obviously exceeded all the water in the alum, while it appeared to fall short of the water and sulphuric acid added together. For if the water amounts to 45 grains (and from Berzelius's experiments it cannot be less); the water and sulphuric acid united amount to 77·8542 grains.

(3.) 100 grains of alum exposed to a strong heat in a wind furnace lost 71·66 grains. This also falls short of the water and sulphuric acid.

Upon examining the alumina which remained when the residual matter of experiment (2) was digested in water; by dissolving it in muriatic acid, and mixing the solution with muriate of barytes, I obtained a precipitate of sulphate of barytes indicating the presence of little more than 1-10th of a grain of sulphuric acid. The alumina from experiment (3) examined in the same way gave no traces whatever of sulphuric acid. The solution made by digesting distilled water on it (which contained the sulphate of potash from the alum) being examined, was found to give a strong purple tinge to cudbear paper. Of course it contained an excess of alkali. It thus became evident that the heat of a wind furnace is sufficient not only to drive off all the water and all the sulphuric acid united to the alumina, but likewise a portion of the sulphuric acid of the sulphate of potash. On precipitating the whole sulphuric acid from this solution by means of muriate of barytes, I found that the quantity of sulphuric acid which it contained was about 0·8 grain below the quantity which the sulphate of potash in 100 grains of alum ought to contain. This 0·8 grain had been dissipated by the heat of the wind furnace from the sulphate of potash, and had rendered it alkaline.

These facts suggested a method of determining the quantity of water in alum, and upon putting it in practice, I found that the process, when repeated carefully, gave me always the very same result. Instead of 100 grains of alum, I employed in preference 60·875 grains.

My method was this: I exposed 60·875 grains of alum to an intense red heat in a wind furnace in a platinum crucible which I had previously weighed. The loss of weight sustained was 43·62 grains. The residual matter in the crucible was digested in distilled water. The clear solution was separated from the alumina by the filter, and the filter was washed with distilled water till the liquid ceased to be affected by muriate of barytes. The solution thus obtained was concentrated on the sand-bath, and then precipitated by muriate of barytes. The sulphate of barytes obtained, after being washed, dried, and heated to redness, weighed 13·28 grains, which is equivalent to 4·504 grains of sulphuric acid. If we add this weight of sulphuric acid to the 43·62 grains driven off by heat, we obtain 48·124 grains as the weight of the whole water and sulphuric acid contained in the 60·875 grains of alum. If from this quantity we deduct 20 grains, formerly shown to be the weight of the sulphuric acid, there remain 28·124 grains of water. A repetition of this experiment gave the very same result.

Should any person think of repeating this experiment, he must be on his guard not to use filtering paper till it has been digested for some time in distilled water; for I was once or twice deceived by using a filtering paper, which exhibited traces of sulphuric acid. I was puzzled at getting more sulphate of barytes than I ought to have had. The excess indeed was very small; but it prevented that exact coincidence between different experiments which I was anxious to obtain.

The reader will please to observe, that 28·124 almost exactly coincides with the weight of 25 atoms of water; for 1·125 × 25 = 28·125. My number is only $\frac{1}{28124}$ less than this quantity. Surely then I am warranted in concluding that 60·875 parts of alum contain exactly 25 atoms, or 28·125 parts of water: 100 parts of alum then contain 46·2012 parts of water, which is 1·2012 more than the quantity detected by Berzelius.

3. *Potash.*—To determine the potash contained in alum, I found that an easier process than that of Berzelius gave results fully as accurate. 100 grains of alum were exposed in a platinum crucible to a moderate heat on the sand-bath till the water of crystallization was dissipated. The crucible was then kept for half an hour in a red heat. Distilled water was now poured upon the mass remaining in the crucible in successive portions,

and digested on it till it ceased to take up any thing. The aqueous solution thus obtained being evaporated to dryness, the sulphate of potash remaining weighed 18·09 grains. On dissolving the sulphate of potash in water, and pouring ammonia into the solution, a slight opalescence was perceptible, indicating that the salt was not absolutely free from sulphate of alumina. It was not possible to collect, far less to weigh, the extremely minute portion of alumina thus disengaged. But by dissolving small quantities of sulphate of alumina in water, and throwing down the alumina by means of ammonia, I was enabled to conclude that the sulphate of alumina mixed with the sulphate of potash from the 100 grains of alum amounted very nearly to 0·02 grain. Deducting this from the 18·09 actually found, there remain 18·07 grains for the sulphate of potash really contained in 100 grains of alum crystals. Now 100 : 60·875 :: 18·07 : 11·0001125. This last number differs so little from 11 that there can be no hesitation in adopting 11 as the true quantity. Had I made my experiments on 60·875 grains of alum instead of 100, and obtained so near a coincidence, I would not have regarded myself as at liberty to consider the weight obtained as differing from 11 grains; because the sources of error are too numerous to make it at all likely that the sixth decimal figure can be depended on.

Eleven grains then is the quantity of sulphate of potash contained in 60·875 grains of alum. Now 11 sulphate of potash are composed of 5 sulphuric acid + 6 potash. Thus exactly one-fourth part of the sulphuric acid in alum is united to the potash. And the weight of potash contained in 60·875 parts of alum is 6, or an atom of potash.

4. *Alumina.*—From the preceding experiments, it is evident that three-fourths of the sulphuric acid in alum are united to the alumina. This in 100 grains of alum amounts to 24·64 grains. I calculated the weight of carbonate of potash, carbonate of soda, and carbonate of ammonia, just sufficient to saturate 24·64 grs. of sulphuric acid. Each of these quantities was added to 100 grains of alum previously dissolved in distilled water, and the whole was well agitated till all action was at an end. By this addition, the alumina was completely precipitated from the solution, while no excess of any of the alkalies could be detected after the precipitation in any of the residual liquids, except of the ammonia, which I had added slightly in excess; from the carbonate containing rather more ammonia than I had supposed it to do. To obtain the whole alumina from each of these liquids, the method which I employed was this: I took three pairs of double filters, each filter in every pair being exactly of the same weight. The two filters constituting each pair were placed the one within the other, and put into glass funnels in the usual way. Into the first pair I poured the liquid containing the alumina separated by means of the carbonate of potash,

that by carbonate of soda into the second pair, and that by carbonate of ammonia into the third. The alumina on each pair was edulcorated by distilled water till the water which passed through ceased to produce any effect on muriate of barytes. The filters were then allowed to dry in the open air. When as dry as they could be made in this way, the two filters constituting each pair were separated from each other. The outermost was put into one of the scales of the balance, and the innermost still containing the whole alumina was put into the other. As the two filters were exactly of the same weight, it was easy to determine the exact weight of the alumina. A portion of the alumina thus weighed was now detached from the filter, and exposed to a strong red heat in a platinum crucible; and from the loss of weight which it sustained, it was easy to deduce the loss of weight which the whole alumina would have sustained had it been subjected to the same process. The following are the results of these experiments:

(1.) The alumina precipitated by the carbonate of potash weighed 24·59 grains. When heated to redness, it was reduced to 10·988 grains.

(2.) The alumina precipitated by the carbonate of soda weighed 24·34 grains. When heated to redness, it was reduced to 10·82 grains.

(3.) The alumina precipitated by the carbonate of ammonia weighed 30·44 grains. When heated to redness, it was reduced to 11·39 grains. It is right to mention that the slight excess of ammonia had dissolved a minute portion of the alumina. This was obtained by evaporation, and is included in the preceding quantity. The liquid precipitated by the carbonates of potash and soda afforded no traces of alumina.

The mean of these three experiments gives 11·066 grains for the alumina contained in 100 grains of alum.

These three experiments were repeated with very nearly the same result; the only difference was, that the alumina precipitated by carbonate of ammonia weighed 11·48 grains instead of 11·39 grains, which was the quantity obtained the first time. This makes the mean of the three last experiments 11·096 grs.

The mean of all the six experiments (which must come very near the truth) is 11·081 grains of alumina in 100 grains of alum. Hence 60·875 grains of alum must contain 6·74555875 grains of alumina. This quantity of alumina was in combination with 15 grains of sulphuric acid, which is equivalent to three atoms of that acid. Now sulphate of alumina, as I have ascertained by experiment, is a white salt, having an acid and astringent taste similar to alum, and reddening vegetable blues as powerfully as that salt. It does not owe this property to any excess of sulphuric acid; for it retains it after repeated digestions in alcohol. Hence there can be no doubt that it is a compound of an atom of acid and an atom of alumina.

The three atoms of sulphuric acid then must be combined with three atoms of alumina. Consequently 6·74555875 must be equivalent to three atoms of alumina; but $\frac{6·74555875}{3} = 2·2485$, a number which would represent the weight of an atom of alumina if my experiments had been perfectly accurate. But it is easy to show that my number is $\frac{1}{1500}$th part too small, and that the true weight of an atom of alumina is 2·25.

For this purpose let us take the constituents of 60·875 grains of alum as determined by the preceding experiments.

Sulphuric acid........	20·000 or	4 atoms
Water...............	28·125	25 atoms
Potash	6·000	1 atom
Alumina..............	6·745	3 atoms
	60·870	
Loss.................	0·005	
Total................	60·875	

There is obviously a loss amounting to 0·005 of a grain. If we add this to the alumina, it will make the three atoms of it to weigh 6·75; and consequently the weight of 1 atom will be 2·25. Now as the weight of an atom of sulphuric acid, potash, and water, is known with precision, it is obvious that the loss can only fall upon the alumina. Hence there can be no doubt that the true quantity of alumina contained in 60·875 grains of alum is 6·75, and that an atom of alumina weighs exactly 2·25. Alum then is composed of

4 atoms sulphuric acid .... =	20·0
3 atoms alumina	6·75
1 atom potash	6·0
25 atoms water	28·125
	60·875

So that the weight of an integral particle of alum is 60·875.

We may represent the composition of alum in a different way, as follows:

3 atoms sulphate of alumina	21·75
1 atom sulphate of potash	11·0
25 atoms water	28·125
	60·875

These proportions are more convenient for calculation than the usual mode of representing the constituents of 100 grains of alum. However, for the sake of those who prefer that method, I shall state the centesimal constituents of alum as follows:

Sulphuric acid	32·8542
Alumina	11·0882
Potash	9·8562
Water	46·2012
	99·9998

Or it may be stated in this way:

Sulphate of alumina	35·72885
Sulphate of potash	18·06975
Water	46·20123
	99·99983

But it is much more convenient in general, because we are not perplexed by a great number of decimal places, to employ in our calculations the weight of an atom of the salt. The atomic weight of an integral particle of any salt never can contain more than three decimal places. When the atoms of water in it are represented by an even number, then the decimal places never can exceed two.

It has been alleged that alum owes its property of reddening vegetable blues to a quantity of bisulphate of potash which it contains; and this opinion has been supported by the following experiment: Mix together solutions of sulphate of alumina and sulphate of potash—a precipitate, it is said, appears. Hence it is alleged that the sulphate of potash is converted into bisulphate of potash, and that the alumina thus partly deprived of acid becomes insoluble, and occasions the precipitate. I have repeated this experiment with all possible care, and with salts in a state of purity. I never could obtain any immediate precipitate whatever; but when the mixed liquid was allowed to remain for 24 hours, there was always a deposit of alum crystals. We have, therefore, no evidence whatever of the presence of bisulphate of potash in alum; and the preceding experiments are quite incompatible with such a supposition.

ARTICLE II.

On certain Saline Solutions which may be cooled without Crystallization; but deposit Crystals when agitated. By Thomas Thomson, MD. FRS. Regius Professor of Chemistry in the University of Glasgow.

It has been long known to chemists that a saturated solution of sulphate of soda in a well corked phial may be cooled down to the common temperature of the atmosphere without the deposition of any crystals. But the moment we take out the stopper, the excess of sulphate of soda separates in a fibrous form, so that the whole liquid assumes the appearance of an opaque solid, while at the same time its temperature rises. I am not aware that any person has hitherto attempted to give a satisfactory explanation of this phenomenon.

There are two salts which possess this property to a considerable extent; carbonate of soda, and sulphate of soda. Probably there are more, but these are the two which I have examined with attention. With sulphate of soda, the phenomenon never fails; but when we employ carbonate of soda, the success depends entirely upon the temperature. If we can cool down the solution below 50°, the success is certain; but at higher temperatures than 50°, the crystals are not deposited immediately, though they generally appear in a few hours. The crystals formed are very different in these two liquids. In the solutions of carbonate of soda, the crystals appear at the surface in the form of small stars, not unlike flakes of snow. These fall slowly through the liquid, giving the appearance of a shower of crystals. The deposition goes on for some minutes, and the crystals accumulate at the bottom of the phial, and at last fill it for rather more than one-third of the portion occupied by the liquid. The sulphate of soda begins likewise to crystallize at the surface of the liquid; but the crystals are so abundant, that the whole surface becomes at once solid, and this crystallization goes on slowly till it reaches the bottom of the phial in about a quarter of a minute. The crystals thus formed put one very much in mind of the fibrous variety of sulphate of lime. The crystals gradually sink towards the bottom of the phial, and in two or three days constitute a solid mass occupying at least four-fifths of the liquid, while the remaining fifth is a clear transparent liquid occupying the upper part of the phial.

To enable us to understand the nature of the phenomenon more accurately, let us examine each of the two solutions a little more closely.

1. Carbonate of soda is a salt composed of

1 atom carbonic acid .... = 2·75
1 atom soda .... = 4·0
11 atoms water .... = 12·375

19·125

When heated, the water of crystallization is sufficient to cause it to run into a liquid. When exposed to the temperature of about 400°, it gradually loses the whole of its water, and is converted into a hard, white, dry, saline mass, which dissolves in water much more slowly than the crystals. It is scarcely necessary to remark, that both the anhydrous and crystallized salts are much more soluble in hot water than cold water.

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I threw the whole of the liquid and crystals upon a cotton cloth. After the liquid had passed through, the cloth containing the crystals was subjected to pressure between folds of filtering paper as long as it imparted moisture to the paper. The cloth with the crystals was then exposed to a gentle heat, which was gradually augmented till all the water of crystallization was driven off. The anhydrous salt obtained in this manner weighed 123·15 grains, or $\frac{1}{3·92}$ part of the salt. Now the water of crystallization belonging to 123·15 grains of anhydrous carbonate of soda is 223·6 grains.

As the crystals were deposited, the temperature of the mixture was augmented by 14°, as accurately as I could determine. The weight of the glass phial in which the solution was kept was 1351·7 grains. But the specific heat of glass is as nearly as possible one-fifth of that of water. Instead of the glass, therefore, we may substitute a quantity of water equal to one-fifth of the weight of the glass, or 270·3 gr. The specific heat of a saturated solution of carbonate of soda is very nearly 0·75. We may, therefore, substitute for the solution a quantity of water weighing just three-fourths of our liquid, or 1879·2 grains. This, with the water representing the glass, makes a total of 2149·5 grains. Now the water of crystallization of the crystals which were deposited (223·6 grains) constitute $\frac{1}{9·61}$ part of the whole.

Now if we suppose that this water during the crystallization of the salt parted with the whole of its latent heat amounting to 140°, and that this was the cause of the augmentation of temperature observed; it is obvious that the temperature of the liquid would have been elevated $\frac{140°}{9·61} = 14·46°$.

Though this is almost half a degree higher than the elevation of temperature which I observed, I have no doubt that the latent heat of the water of crystallization of the salt deposited was the sole source of the heat observed. For my experiment was exposed to two sources of error, which I could not completely obviate, and both of which had a tendency to make the latent heat of the water of crystallization appear higher than the augmentation of temperature observed.

1. The salt took nearly three minutes before it was all deposited, and during all that time, the temperature of the liquid was augmenting. But as it was about 14° higher than the surrounding atmosphere, it is obvious that a portion of the heat must have been dissipated before it reached its maximum; consequently the augmentation of temperature which I observed must have been a little less than the truth.

2. The crystals of carbonate of soda which I collected on the cotton cloth were exceedingly small, and they contained a great

deal of the liquid within their interstices. This liquid I endeavoured to get rid of by means of filtering paper, which imbibed it; but after the crystals were rendered as dry as possible by this means, they were still far from being perfectly so. A small portion of liquid must still have been contained within the interstices of the crystals. Now this liquid still held a considerable quantity of carbonate of soda in solution. Hence the weight of anhydrous carbonate which I obtained must have somewhat exceeded the truth. Had the quantity been such that the water of crystallization which it contained amounted to $\frac{1}{10}$ instead of $\frac{1}{9 \cdot 61}$, then the elevation of temperature observed would have been exactly equal to the latent heat.

I have no doubt that these two sources of error taken together are the cause of the small difference of half a degree between the theoretical and practical results.

When the liquid from which the crystals of carbonate of soda had been deposited was set aside for two or three days, an additional crop of crystals separated from it. These crystals were weighed, and found to amount to 214·6 grains, which is equivalent to 75·89 grains of the dry salt. Thus it appears that 8-13ths of the surplus salt are deposited immediately in crystals, while the remaining 5-13ths remain in solution; but are notwithstanding deposited in the course of two or three days in the state of crystals. Thus the liquid was at last reduced to the state of a saturated solution at 50°.

2. Sulphate of soda is a salt composed of

1 atom sulphuric acid .... =	5·0
1 atom soda.	4·0
10 atoms water.	11·25
	20·25

When heated moderately, its water of crystallization is sufficient to cause it to liquefy. Gay-Lussac has shown that water of the temperature 106° dissolves a maximum of this salt, and that the solubility diminishes when the temperature is increased. I have reasons for believing that carbonate of soda is distinguished by a similar property, but its maximum point of solubility is as high as 120°. At that temperature water is capable of taking up a greater quantity of the anhydrous carbonate than at 160°.

To form a solution of sulphate of soda capable of crystallizing when agitated, we have only to dissolve 51 parts of the crystallized salt in 49 parts of water; or, which is the same thing, 22·44 parts of the anhydrous salt in 77·56 parts of water; or 28·91 parts of the anhydrous salt in 100 parts of water. This constitutes a saturated solution at the temperature of 88·25°.

If we attempt to make a solution containing a greater proportion of salt than that just stated, we shall find that it cannot be cooled down without depositing crystals. The specific gravity of the above solution at 87° is 1·1995. I have never been able to determine the specific gravity at 60°, but think it likely that at that temperature it would be 1·228.

When the above solution is cooled down to about 50° in a well-corked phial, if we draw the cork, a copious deposition of fibrous crystals make their appearance on the surface of the liquid, and the crystallization in about half a minute extends through the whole liquid, converting it into a semitransparent, fibrous white solid, while in the mean time the temperature of the whole rises, as nearly as I have been able to determine, 24° of Fahrenheit.

One hundred grains of the residual liquid after the separation of the crystals being evaporated to dryness left 8·62 grains of anhydrous sulphate of soda. Hence it is obvious that this liquid is a compound of 100 parts of water + 9·43 parts of anhydrous sulphate. We see from this that very nearly two-thirds of the whole salt in solution had been deposited in crystals by an instantaneous crystallization.

In an experiment which I made, the weight of the glauber salt solution was 2118 grains; and the weight of the phial in which it was, amounted to 1032 grains.

The whole sulphate of soda in the liquid, supposing it in a crystallized state, was 1070 grains. Of this quantity, two-thirds, or 713 grains, were deposited in fibrous crystals. Now the water of crystallization in this quantity of salt amounts to very nearly 399 grains.

The specific heat of a solution of glauber salt is about 0·73. We may, therefore, consider the 2118 grains of the solution as equivalent to 1546 grains of water. If we reckon the specific heat of the phial 0·2, we may consider it as equivalent to 206 grains of water, both of which together amount to 1752 grains. Now 399 grains (the water of crystallization of the salt) constitute $\frac{1}{4\cdot39}$ of 1752 grains. Hence we obtain the amount of the heat evolved by the water of crystallization. If we multiply 24°, (the number of degrees of rise of temperature) by 4·39, the product is 105·36°.

It would appear from this, that the water of crystallization does not, in the act of solidification, part with the whole of its latent heat, but only with about three-fourths of it. But a phenomenon which always has taken place in all my experiments on this subject (and they have been numerous) enables us to account for this apparent diminution of heat in a satisfactory manner. When the phial containing the fibrous crystals mixed with liquid is set aside for some days, the crystals subside, and a portion of clear liquid swims over them. Now if we examine the crystals, we shall find that the lower part of them is still fibrous; but just under the liquid portion, there is a stratum of regular prismatic crystals of sulphate of soda. It is obvious from this, that the whole salt did not separate in crystals at first. An additional quantity was obviously deposited afterwards. Hence I overrated the weight of the salt deposited in a fibrous state, and consequently the weight of its water of crystallization. Nor is it difficult to see the reason of this. The increased temperature of the liquid (amounting to 24°) will of course prevent the whole surplus salt from being deposited till the liquid cools. I have not been able to determine the weight of this second crop of crystals (as I did with respect to the solution of carbonate of soda); but from their appearance, they cannot amount to a smaller proportion than one-fourth of the whole mass of crystals deposited. Hence we have reason to conclude, that the weight of the fibrous crystals at first deposited was only 530 grains, instead of 713 grains. Now the water of crystallization of this quantity of salt is about 300 grains, constituting $\frac{1}{5 \cdot 83}$ of the whole mass. Now $5 \cdot 83 \times 24 = 139 \cdot 92$.

From this statement there seems no reason to doubt that the water of crystallization of the salt which crystallizes gives out its latent heat, and that this evolution is the cause of the augmentation of temperature observed, though the difference of solubility has not been hitherto accurately determined. It will appear from what follows, that at the temperature of 50°, 100 parts of water dissolve about 14·5 parts of the dry salt, which is equivalent to 48·01 parts of the salt in crystals; while at the temperature of 98°, 100 parts of water take up 23·69 parts of the dry salt, equivalent to about 67·11 parts of the crystals.

To form a solution of carbonate of soda which deposits crystals when cooled down to 50° (on taking out the stopper) we have only to dissolve an ounce troy of the dry salt in 4·22 ounces of water. Now this is the same thing as dissolving 23·69 parts of the dry salt in 100 parts of water. When the above solution is cooled down to 50°, and the cork of the phial is drawn, a copious precipitate of small crystals in stars takes place, and the temperature of the solution is elevated, as nearly as I could determine, 14°.

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ARTICLE III.

Meteorological Results deduced from Diurnal Observations kept at the Apartments of the Royal Geological Society of Cornwall for 1821. By E. C. Giddy, Esq. Curator. (Communicated by Dr. Forbes.)

1821.	BAROMETER.												
Months.	Maximum.	Minimum.	Mean of the max.	Mean of the min.	Mean of the month.	Monthly range.	Mean daily range.	Greatest daily range.	True maximum.	True minimum.	Mean of true max.	Mean of true min.	True mean of the month.
January .	30·50	28·82	29·71	29·61	29·660	01·68	00·10	00·59	30·434	28·776	29·668	29·556	29·612
February.	30·46	29·00	30·05	29·99	30·020	01·46	00·05	00·28	30·409	28·958	30·008	29·948	29·978
March...	30·07	28·80	29·57	29·44	29·505	01·27	00·13	00·44	30·010	28·752	29·506	29·380	29·443
April . . .	30·03	29·18	29·57	29·48	29·525	00·85	00·09	00·30	29·961	29·096	29·516	29·417	29·466
May	30·12	29·12	29·72	29·63	29·675	01·00	00·08	00·28	30·048	29·057	29·648	29·558	29·604
June....	30·12	29·48	29·90	29·84	29·870	00·64	00·05	00·17	30·036	29·402	29·816	29·753	29·784
July	30·06	29·33	29·81	29·76	29·785	00·73	00·05	00·20	29·964	29·234	29·714	29·661	29·687
August..	29·96	29·45	29 80	29·76	29·780	00·51	00·04	00·11	29·858	29·342	29·695	29·652	29·673
Sept.....	30·10	29·40	29·76	29·70	29·730	00·70	00·07	00·31	29·998	29·304	29·664	29·601	29·632
October..	30·13	28·96	29·76	29·70	29·730	01·17	00·08	00 22	30·046	28·900	29·682	29·622	29·652
Nov.....	30·10	29·12	29 69	29·59	29·640	00·98	00·11	00·35	30·031	29·042	29·618	29·518	29·568
Dec.....	30·00	27·85	29·38	29·22	29·300	02·15	00·15	00·52	29·934	27·806	29·320	29·160	29·240
Annual means,&c	30·50	27·85	29·72	29·64	29·680	13·14	00·08	00·59	30·434	27·806	29·654	29·569	29·611

1821.	DAY AND NIGHT THERMOMETER.								COMMON THERMOMETER.												
Months.	Maximum.	Minimum.	Mean of the max.	Mean of the min.	Mean of the month.	Monthly range.	Mean daily range.	Greatest daily range.	Min. at 8, a.m.	Max. at 2, p. m.	Min. at 8, p.m.	Mean at 8, a. m.	Mean at 2, p.m.	Mean at 8, p.m.	Mean of the month from the three preceding columns.	Monthly range.	Mean daily range.	Greatest daily range.	Mean at 10, a. m.	Mean at 10, p. m.	Mean of the month from the two preceding columns.
January	57	26	47	42	44·5	31	6	12	26	55	29	44	47	44	45·5	29	4	9	0	0	0·0
February	52	32	45	38	41·5	20	7	16	36	52	36	42	46	42	43·5	16	4	8	0	0	0·0
March...	54	34	50	43	46·5	20	7	13	40	56	39	47	52	47	49·0	17	6	12	47	45	46·0
April....	64	37	55	45	50·0	27	10	15	45	64	42	51	55	49	52·0	22	6	10	51	47	49·0
May	61	40	56	45	50·5	21	10	16	46	62	44	53	56	51	53·5	18	6	10	51	48	49·5
June....	67	43	62	51	56·5	24	11	15	50	66	50	57	61	57	59·0	16	6	10	58	53	55·5
July....	71	51	66	55	60·5	20	11	18	56	70	55	60	65	59	61·5	15	6	10	60	55	57·5
August..	73	55	68	58	63·0	18	10	15	58	71	57	62	67	62	64·0	14	5	9	64	60	62·0
Sept.....	72	49	65	56	60·5	23	9	15	54	71	54	60	64	60	61·5	17	5	9	62	58	60·0
October..	64	41	58	50	54·0	23	8	16	46	64	44	54	57	54	55·0	20	3	9	55	52	53·5
Nov.....	60	40	55	47	51·0	20	8	17	41	60	41	51	53	52	52·0	19	4	9	51	50	50·5
Dec.....	56	36	51	44	47·5	20	7	15	38	55	40	47	49	47	48·0	17	4	9	48	48	48·0
Annual means,&c	73	26	56	48	52	22·3	9	18	26	71	29	52	56	52	53·5	16·6	5	12	54	53	53·5

1821.	WIND AND WEATHER.														RAIN IN INCHES.		
Months.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Var.	Calm.	Brisk.	Boisterous.	Prevailing.	Dry days.	Wet days.	Morning.	Evening.	Total.
January .	0	6	4	5	5	2	8	1	0	0	4	0	W	15	16	0·94	1·12	2·06
February.	2	0	12	8	1	0	4	1	0	0	6	3	E	25	3	0·32	0·10	0·42
March...	3	0	0	3	4	7	6	8	0	0	28	2	NW	14	17	1·67	1·82	3·49
April....	1	4	0	6	2	4	7	6	0	0	9	3	W	12	18	0·97	0·70	1·67
May....	1	3	1	5	4	2	8	7	0	0	13	2	W	16	15	1·74	0·45	2·19
June....	3	2	4	6	4	1	5	1	3	1	1	0	SE	21	9	0·52	0·74	1·26
July....	3	0	0	3	3	3	6	9	3	1	6	0	NW	21	10	0·81	0·18	0·99
August..	1	0	0	11	1	4	7	6	0	1	3	0	SE	17	14	3·49	0·51	4·00
Sept....	0	0	0	3	2	12	6	5	0	1	10	1	SW	14	16	2·00	1·19	3·19
Octob[illegible].	5	1	0	3	3	13	3	3	0	0	15	3	SW	13	18	2·57	0·98	3·55
Nov....	1	0	0	3	8	12	2	3	0	1	16	5	SW	9	21	1·64	1·47	3·11
Dec.	0	0	0	3	4	15	3	6	0	0	19	9	SW	3	28	4·49	2·09	6·58
	20	16	21	59	42	75	65	56	6	5	130	28	SW	180	185	21·16	11·35	32·51.

Barometer.

Highest, Jan. 23.	Wind, E	30·50
Lowest, Dec. 28.	Wind, SE.	27·85

Register Thermometer.

Highest, Aug. 22.	Wind, SE	73
Lowest, Jan. 1 and 2.	Wind, NE and SE	26

Common Thermometer.

Highest, Aug. 25.	Wind, S	71
Lowest, Jan. 1.	Wind, NE	26

Observations.

Jan. 1.—Weather fine and clear, with a sharp frost; 2d and 3d a fall of snow, which continued at intervals throughout those days, but disappeared on the night of the 4th; from 6th to 20th, rainy, showery, and misty. The remainder of the month very fine.

Feb.—This month, with the exception of three days, was very fine, and remarkable for the height of the mercury in the barometer, which stood above 30·00 for *twenty-three successive days.* There are but *two instances* in the last three years, in any month, wherein the mercury stood above 30·00 more than *eleven days,* and those not in succession.

March.—In general, wet and stormy. On the 26th, it blew a strong gale from the SW, with heavy rain; between eight and nine o'clock at night, the wind suddenly shifted to the NW, blowing a hurricane.

April.—This month, like the former, was wet and stormy; some heavy hail showers, with thunder and lightning.

May.—Also wet and unseasonable.

June.—In general, a very fine month.

July.—Like the former.

August, September, and October.—Almost constant rain, with heavy gales of wind, accompanied by thunder and lightning.

December.—This month might have been added to the former three, but it was necessary to remark, that on the 28th, the mercury in the barometer fell to 27·85, being 00·43 lower than it had fallen for the last four years (on the 4th of March, 1818, it stood at 28·28), and it has been asserted by persons who have been in the constant habit of observing the fluctuations of the mercury for nearly 40 years, that they never saw it so low. It may be proper to remark, that the observations refer to the common upright barometer.

Rain, &c. for Three Years.

	Wet days.*	Dry days.	Rain in inches.	Prevailing wind.
1819.	181	184	23·83	W
1820.	137	229	16·15	NW
1821.	185	180	32·51	SW
Means	167	198	26·165	

Note by Dr. Forbes.—The rain-guage made use of in Mr. Giddy's observations is placed on the top of a chimney (not overlooked by any neighbouring buildings) about 45 feet from the ground. The following † are the average results of two guages (the one kept by Mr. Boase, the other by myself) placed on the ground, several hundred yards apart, and also some hundred yards respectively, from the site of Mr. Giddy's. A comparison of their results (which accord very exactly with those of Mr. G.'s), shows their coincidence with former observations of a like kind (see Howard's Climate of London, vol. i. tab. 66), and points out the absolute necessity of noting the local circumstances of the pluviameter in every tabular record of the rain. —J. F.

ARTICLE IV.

Analysis of a Native Phosphate of Copper from the Rhine.

By Francis Lunn, BA. FRS. of St. John's College, Cambridge.‡

AMONG the analyses of Klaproth § is one of a phosphate of copper from the Firneberg, near Rheinbreitenbach, on the Rhine: the mineral had long been mistaken for malachite, from its external resemblance. The German chemist obtained as his result

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* Wet days comprehend rainy, showery, snowy, and those in which hail fell.

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March	3·53
April	2·66
May	3·52
June	1·55
July	1·63
August	4·71
September	4·56
October	5·57
November	5·19
December	9·51
Total	42·43

Two first months of the year not noted.

‡ From the Transactions of the Cambridge Philosophical Society for 1821.

§ Beiträge zur Chemiscen, iii. 206.

Oxide of copper	68·13
Phosphoric acid	30·95

And this analysis has been adopted by Haüy, Brogniart, Thomson, Jameson, Phillips, and in short copied into every mineralogical classification which has appeared. Its accuracy has been doubted,[*] it is true: for water to the amount of 15 per cent. is overlooked; but the rarity of the substance has prevented chemists from subjecting it to a new examination.

Having lately received some copper ores brought from a mine at Erpel, near the town of Bonn,[†] among these were specimens of the mineral in question. In mineralogical characters it agrees with the description of Klaproth.

In colour it is emerald green, but shaded and streaked with black green, and to this colour the external natural surface approaches; it is opaque, its powder is verdegris green, it has a diverging striated texture, and a silky lustre; the specific gravity of one very pure fragment was 4·2; its hardness is rather beyond that of malachite. It was in no instance crystallized, although on the external surface of some specimens an imperfect tendency to crystallization was perceptible. It occurs massive in a white opaque quartz rock, in places slightly tinged by oxide of iron; and it is soluble in nitric acid. By exposure to a red heat in a close crucible, it becomes of a dark olive-green colour, and the powder increases considerably in bulk. Before the blowpipe on charcoal, it readily fuses into a reddish black slag, adhering to the charcoal, and by the addition of carbonate of soda, it is reduced to a bead of pure copper.

In some specimens, it is accompanied by crystals of phosphate of lead.

Although the elements of this mineral are not numerous, yet wherever phosphoric acid enters, considerable caution is necessary to ensure correct analytical results: nothing can more fully prove this than the discordancies between the results obtained by two of the most expert analysts, Professors Thomson and Berzelius, and yet both have exerted their utmost skill on this very subject.

It was necessary to make several previous trials to find out a precipitant which might be depended upon; or rather, to find out the mode of using any of the old ones which would produce accordant results. These trials were made upon anhydrous phosphate of soda by barytes, lime, and the salts of lead. I need not repeat a tedious course of experiments, but may mention the results: the objection to the earthy salts is that unless the solution be most strictly neutral, or even rather alkaline, a very small quantity of the phosphoric acid enters into insoluble combination; these, therefore, would not answer the intended purpose, because at the very point when the reagent would be useful, the original salt was itself precipitated.

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[*] L'analyse de M. Klaproth n'indique pas que l'eau entre dans la composition de ce phosphate, cependant il en contient une quantité assez considérable; &c. &c. Cette circonstance jette doute sur l'exactitude de l'analyse de Klaproth; elle mérite bien d'etre répétée. Berzelius. Nov. Syst. p. 246. Paris, 1819.

[†] For these very fine specimens, I am indebted to George Samuel Kett, Esq. Brooke House, Norfolk.

To the salts of lead then we must have recourse: the muriate appears to have the preference with Berzelius,* and with this salt accordant results may be obtained; but both the saline solutions must be most strictly neutral, and the very low degree of solubility of muriate of lead after it has once been crystallized, is a considerable practical inconvenience. With the nitrate of lead I could obtain unvarying results; it is easily crystallized, and, when carefully washed and redissolved, is perfectly neutral, and of high solubility.

In both the above methods, it is adviseable to ensure accuracy, that no more of the solution of the salt of lead be added than is necessary to separate the phosphoric acid; and the precipitate must be boiled in water, by which means the combination of acid mentioned by Berzelius † may be avoided.

Analysis.

A portion of the mineral free from any foreign ingredient was reduced to a fine powder: this, after being dried at the temperature of 212°, was of a verdegris green colour, and weighed 28·7 grains. By subjecting it to a low red heat in a platinum crucible, it became olive-green, more bulky, and lost 2·15 in weight, which was water driven off; it was not adviseable to let it remain long at that heat, for it is capable of being volatilized, which appeared by some condensing on the lid of a crucible. The whole was now dissolved in dilute nitric acid, and formed a clear blue solution; from this as much water and excess of acid was driven off as possible by a long continued gentle heat. The whole was now very carefully neutralized with a weak solution of potash so as just to avoid the reprecipitation of the salt: nitrate of lead from fresh dissolved crystals was carefully added, avoiding excess, which was shown by the clear liquor above the white precipitate undergoing no change from sulphate of soda, nor hydriodic acid. The precipitate boiled, well washed, and dried, was, after a red heat = 31·23 grains, equivalent to 6·246 of phosphoric acid.

A solution of caustic potash was added in excess, and boiled upon the black precipitate formed; this when separated and dried at a heat below redness weighed 18·1 grains, and was copper in the same state of oxidation as in the mineral.‡

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* Annales de Chimie, ii. 159. † Ibid.

‡ The analysis was also accomplished in another manner, by adding hydrate of ammonia in such excess as to redissolve the precipitate at first formed. The phosphoric acid was then precipitated by cautiously adding nitrate of barytes, and after the liquor had been rendered acidulous by sulphuric acid, the copper was separated by a plate of iron. The method described in the text has, however, practical advantages.

Hence

Phosphoric acid	6·246
Peroxide of copper	18·1
Water	2·15
	26·496
Loss	2·304
	28·8

Now this loss would appear considerable, if we do not take into account the impossibility of having driven off all the combined water, for reasons above stated. If we consider that loss to be water, the result will stand thus:

Phosphoric acid	6·246	=	21·687	per cent.
Peroxide of copper	18·1	=	62·847	
Water	4·454	=	15·454	
	28·8		100·000	

Now it is fair, at least, to compare all theory with experimental results; if we consider the mineral as composed of one atom of phosphoric acid, one atom of peroxide of copper, and two atoms of water, the quantities per cent. will stand as below; and by the side I have placed the experimental result for comparison.

	Theoretical composition.	Experimental result.
Phosphoric acid	22·222	21·687
Peroxide of copper	63·492	62·847
Water	14·285	15·454

It will be seen that the difference is in no case equal to unity except in the water.*

If we were to represent the constitution of this mineral by the symbols of Berzelius, which, being derived from the Latin, are more general than the English initials of Thomson, but adopting the opinion of the latter with regard to the constitution of phosphoric acid,

Its chemical sign would be $\ddot{C}u\,\ddot{P} + 2\,Aq$.
Its mineralogical ... ... $Cu\,P + 2\,Aq$.

There can be no doubt of Chenevix's artificial phosphate being a biphosphate, as stated by Thomson †; and it is rather singular that a neutral combination which has not hitherto been formed in the laboratory of the chemist, should be the very substance formed by a natural process in the earth.

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* Throughout these calculations I have made use of the atomic weights recently laid down by Thomson, because in some trials of verification I found them to accord best with experiment.

† Thomson's System of Chemistry, vol. ii. p. 607. Fifth Edit.

ARTICLE V.

Remarks on the Geology of the Cliffs at Brighton.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Nov.* 1821.

I AM induced to send you some observations on the geological features of the cliff at Brighton, which I have extracted from notes made at that place in 1817, as I find that some very extraordinary views have been entertained of the relations of the strata there visible.

I refer particularly to an account by J. F. Daniel, Esq. FRS, published in the fourth volume of the Journal of Science, edited at the Royal Institution, which "records some hitherto unnoticed combinations and positions which materially affect our hitherto received notions of the comparative ages of these upper formations." These are the author's words, and he describes (at about half-way between Brighton and Rottingdean) the very remarkable appearance of a bed of *loose pebbles* in the *solid chalk*, and veins of flint passing from one part of the chalk to another *through the bed of pebbles* without suffering any fracture or dislocation.

This account was published, I believe, in 1818. In the spring of this year in a lecture at the Royal Institution, I heard these assertions argued upon, and illustrated by drawings. Entertaining, however, doubts as to their accuracy, I hope that you will not deem the description which I send superfluous. I may be in error certainly, but as what Mr. Daniel has described is, if correct, of so much importance in geology, the publication of my notes may induce some of the many visitors to Brighton to undertake even a toilsome walk of two or three miles along its shore of loose shingles to ascertain the truth between the conflicting statements.

It will be observed that the very spot which I have described (that is, half-way between Brighton and Rottingdean), as having the cliff formed entirely of the solid chalk, is that where these extraordinary appearances are said to occur.

I may observe in favour of the view I have taken, that Mr. Webster, in his excellent paper on the Strata lying over the Chalk, published in the second volume of the Transactions of the Geological Society, though he describes the peculiar structure of the cliff at Brighton, takes no notice of the remarkable appearances that Mr. Daniel and others dwell upon so much.

The observations that I send, it should be noticed, were made previously to Mr. Daniel's publication. Your obedient servant,

INDAGATOR.

On Sept. 24, 1817, I examined the structure of the cliff at Brighton. From the dirt and rubbish thrown over it, it is impossible to make any observations on the west part of the town, nor is the structure clearly to be perceived on the east till you arrive at the last groin, which is near the termination of the houses on the east cliff.

Here there is a passage cut in the cliff to descend to the shore, and a little in advance of this a good idea may be gained of the structure of the whole of the cliff between this place and Rottingdean.

From the top to about four feet above the level of the shingle (as it then was) the cliff consists of fractured chalk flints intermixed with small, mostly rounded, fragments of chalk, cemented together by a very pale ferruginous clay; the cohesion of these materials, though not very firm, is sufficiently strong to make it difficult to pull out a projecting flint by the hand, and also to allow the cliff to be absolutely perpendicular, which is mostly the case: the fragments of flints, though they appear to have been subjected to the action of water, are nevertheless by no means rounded; they are merely deprived of their sharp edges and angles.

Under this stratum, which, as I have said, occupies the whole of the cliff to within about four feet of the level of the shingle, is a bed or layer of perfectly rounded pebbles; they appear to be mostly chalk flints, are quite loose, and rest upon a thin layer of fine silicious sand, and this again rests upon the solid chalk. The latter circumstance cannot, however, at present be seen till you have advanced about a mile east from this spot.

These rounded pebbles are mostly of a large size, and have no intermixture of clay or other substance to bind them together: this may be said generally of the bed. In several spots, however, and particularly a little east of the groin in the upper part of the bed, the interstices are filled up by calcareous matter in a state of very distinct crystallization: hence these pebbles falling from the cliff, form masses of considerable firmness; in other parts, the calcareous matter is in an earthy state: further to the eastward, they are not unfrequently mixed with clay or sand, but still continue loose.

This bed may be distinctly traced to within about one-eighth of a mile of Rottingdean; it may always be distinguished from the superior stratum by the rounded form of the pebbles; it is about six feet in thickness, and from this it does not vary, except near its termination, and in one or two other places. After continuing for about two miles from Brighton, on a level with the present bank of shingle, it begins to rise very gradually, and the solid unaltered chalk appears on which it is seen to rest, except where the thin layer of fine sand occurs, and this is in some places mixed up with the pebbles. About a mile and an half from Rottingdean, where the bed is about four feet above the level of the shore (these four feet consisting of solid chalk) large rounded pieces of chalk begin to occur in it, and these gradually increase so much, that in some places they form the greater part of the bed, and are of a very large size. This intermixture continues for about a mile; the bed then gradually becomes thinner, and less regular, appearing to have some intermixture with the upper stratum, and, continuing to get thinner and thinner, is lost about one-eighth of a mile from the ravine of Rottingdean.

In some places, a good deal of clay is mixed with the pebbles; in others, they are small, but always preserve their character of roundness. I found a rounded piece of granite among them, and saw several rounded fragments of the primitive rocks lying on the shore which were probably all derived from this source. I likewise found a bone of an animal, of the class mammalia, in two places distinctly imbedded in the pebbles: in both cases, it was in a very soft and decomposed state. Pieces of argillaceous iron stone are not of unfrequent occurrence in it.

The frequent falling of the cliff on this shore is I think to be almost entirely attributed to this loose bed of pebbles, near Brighton, where it is on a level with the shore. It is readily washed away at spring tides, and the cliff undermined, but that part of the cliff on which the town stands is now well defended by a low wall built against it: this covers the bed of loose pebbles, and prevents the sea from undermining the cliff. Towards Rottingdean, where it is elevated by the solid chalk above the reach of the waves, the shore is much narrower; but even here, the action of the weather causes the pebbles frequently to fall out, and deprives the upper part of the cliff of its support: hence there is at this part a projecting ledge of chalk about four feet in height, which continues to resist the sea, though the cliff above it has fallen away.

The stratum which forms the upper and main part of the cliff is tolerably uniform throughout, merely varying in this; that in some parts the flints are more abundant, but always of the angular description above mentioned; in others, the fragments of chalk and agglutinating clay are most predominant, sometimes to the total exclusion of the flints. At about one-eighth of a mile from Rottingdean, the solid chalk is seen to form the whole of the cliff, but it is very difficult to say at what exact point the debris ceases and the chalk begins, owing probably to the washing down of the surface by the rains, which, in many parts, conceals the real structure of the cliff.

Although I have described this stratum and the bed of pebbles as continuing the whole way from Brighton to Rottingdean, yet it must be particularly noticed that about half way between the two places, for about 100 yards, the cliff is formed entirely from top to bottom by the solid chalk. On the west side, the bed of pebbles is seen gradually to cease. On the east, it disappears under masses which have fallen from the upper parts of the cliff: at this part, therefore, not only the bed of loose pebbles, but the upper and thick stratum of angular flints and clay, are entirely wanting.*

At that part of the cliff which is exactly opposite the end of the New Steyne, the workmen were forming a descent to the shore, and this operation showed that the structure of the cliff was precisely the same here as to the eastward. About half way down, a circular hole had been dug in the debris of chalk and angular flints, and passed through the bed of loose rounded pebbles into the chalk on which the bed was seen to rest.

I could only see a section of the cliff on the west side of the town in one spot, and that near its termination; it was there composed entirely of angular fragments of chalk flints. It is so low that I suspect if the bed of loose pebbles extended so far, and kept the same elevation it has on the east, that it must be seen here.

Above the cliff I could not see the slightest indication on the surface of the junction of the debris with the chalk; it certainly does not extend far inland; for at the west end of the town, there is very near the shore, a clay from whence they make bricks. At the church, the chalk is close to the surface, and on the opposite side of the valley, it is seen at a less elevation; and between the town and Rottingdean, there are several indications of the chalk from within half a quarter of a mile to half that distance from the edge of the cliff.

It is remarkable that this stratum of debris externally conforms to the various undulations of the chalk surface to which it is united; so that from external appearances, no alteration of the substratum would be suspected.

At low water, the solid chalk may be seen forming the shore all the way between Brighton and Rottingdean.

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* The following is Mr. Daniel's account of (as I suppose) this spot: "About half way between Brighton and Rottingdean, the cliff presents some very curious and important particulars. The upper bed, which has been assuming by gradual degrees more and more the characters of chalk, is decidedly chalk, and towards the top contains two horizontal veins of thin flint. The bed of shingles suddenly contracts to the width of a few inches, but maintains its situation and characters uninterrupted. The lower bed of chalk is intersected by veins of flint, which here traverse the bed of shingles, and continue their course through the upper bed till they reach the horizontal veins before described."

ARTICLE VI.

On the Formation of Ice in the Beds of Rivers. By Thomas M'Keever, MD. Assistant to the Dublin Lying-in Hospital.

(To the Editor of the *Annals of Philosophy.*)

THE numerous and formidable difficulties attendant on an explanation of the principles which, under particular circumstances, occasion the deposition of an icy incrustation in the beds of lakes and rivers, have induced many philosophers to consider the circumstance as highly improbable; while others have gone the length of altogether denying its occurrence. We are, however, no longer dependent on the casual information afforded by uninformed persons for proofs of the fact; the personal observations of several very eminent chemists having placed its existence beyond the possibility of all doubt.

Mr. Leslie, in a note prefixed to his very interesting work on heat, tells us that many of the rivers in Siberia and Switzerland are found to have their beds lined during the greater part of the year with a thick crust of ice. Saussure describes a similar appearance in the lake of Geneva. Mr. Garnet, in a late number of the Quarterly Journal of Science and of Arts, gives an accurate and minute account of this singular phenomenon, and mentions one place in particular where it may be observed in a very striking manner.

"On the river Wharfe, near Otley, in the West Riding of Yorkshire," Mr. G. informs us, "there is a weir or mill dam, the structure of which is of hewn stone, forming a plane, inclined to an angle of from 35° to 50° fronting the north, and extending from W. to E. to the length of 250 or 300 yards. When the wind suddenly shifts from SW. to NW. and blows with great impetuosity, accompanied with severe frost, and heavy falls of snow, the stone which composes the weir soon becomes encrusted with ice, which increases so rapidly in thickness, as in a short time to impede the course of the stream that falls over it in a tolerably uniform sheet, and with considerable velocity; at the same time the wind blowing strongly from the NW. contributes to repel the water, and freeze such as adheres to the crust of the ice, when its surface comes nearly in contact with the air. The consequence is, that in a short time the current is entirely obstructed, and the superincumbent water forced to a higher level. But as the abovementioned causes continue to act, the ice is also elevated by a perpetual aggregation of particles, till by a series of similar operations, an icy mound or barrier is formed, so high as to force the water over the opposite shore, and produce an apparent inundation. But in a short time, the accumulated weight of a great many thousand cubic feet of water presses so strongly against the barrier as to burst a passage through some weak part, through which the water escapes, and subsides to its former level, leaving the singular appearance of a wall or rampart of ice, three or four feet high, and about two feet in thickness, along the greatest part of the upper edge of the weir. The ice composing this barrier, where it adheres to the stone, is of a solid consistency, but the upper part consists of a multitude of thin laminæ, or layers resting upon each other, in a confused manner, and at different angles of inclination, their interstices being occupied by innumerable spiculæ, diverging, and crossing each other in all directions. The whole mass resembles in its texture the white and porous ice, which may be seen at the edge of a pond or small rill where the water has subsided during a frost."

A variety of hypotheses have been framed with the view of accounting for this curious phenomenon, all of which, however, I think I may with confidence assert, are either inadequate to an explanation of the facts, or are altogether inconsistent with the well-established doctrines of chemistry. As the point is, therefore, still open for discussion, I beg leave to state in a very few words in what manner I conceive the deposition to take place.

While reading Mr. Garnet's paper, I was very forcibly struck with the peculiar circumstances under which he states this incrustation to take place: thus he tells us that ice of this description is seldom seen adhering to any substance, except rock, stone, or gravel; and that it is always found in greatest abundance in proportion to the magnitude and number of the stones composing the bed of the river combined with the velocity of the current; as also that it abounds most in rough and rapid places, and that he has never observed it where mud or clay is deposited. Now it has occurred to me that, perhaps, the formation of ice in those situations may be owing to the same causes that give rise to the deposition of dew and hoar frost on grass, twigs, and other fibrous substances; namely, by their possessing a greater radiating power, by which they are enabled to discharge a larger quantity of heat from their surface. The roughened surfaces of the stones I conceive to operate in the same way as the vegetable fibres do, in a clear, unclouded atmosphere, by allowing each "affluent" wave* to come in closer proximity with the surface, and thus favour the discharge of caloric from the bed of the river. That none appears where mud or earth is deposited, I should suppose to be owing to their presenting a comparatively smooth surface, in consequence of which, the stratum of incumbent fluid is prevented coming into such close contact as if a rugged one were presented. Just in the same manner as if we were to take a highly polished globe of silver, and fill it with hot water, it will take suppose 20 minutes to cool down 10 degrees; but if its surface be scratched with sand paper *in one direction,* it will now cool down the same number of degrees in half the time. The striated surface of the metal allowing of a closer, though still partial contact with the bounding atmosphere, is thus brought to a state more favourable for exciting energetic pulsations. On the same principle, a thin covering of muslin, or even of flannel, instead of retarding the escape of heat, as *a priori* we should suppose, does actually favour its more rapid discharge.

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* Dr. M'Keever adopts Mr. Leslie's theory of radiation.—*Ed.*

The NW. wind probably acts by its greater degree of cold, causing (at least until the temperature of the entire mass is reduced to 39) a constant precipitation of chilled particles from the surface to the bed of the river.

How the rapidity of the currents can hasten the effect, it is difficult to say; unless on the same principle that a strong breeze accelerates refrigeration in atmospheric air, so in like manner the rapid current affording a constant supply of water at a lower temperature may cool down the bed of the river with greater rapidity, and thus bring it to a condition more favourable for the production of this icy crust.

It will in all probability be objected to the suggestions I have here thrown out, that they are in direct opposition to the observations of Prof. Leslie, who asserts, that when the cannister, reflector, and differential thermometer, were plunged into water, that no radiation could be observed, and hence this ingenious philosopher concludes, that no radiation will take place, except when the radiating body is surrounded by an elastic medium. I may remark, however, that the experiments which he adduces in support of this opinion are by no means decisive of the point. Substances, as Dr. Thomson very accurately remarks, cool so rapidly when plunged into water that there is hardly time for the differential thermometer to be affected; besides that, the heat could scarcely accumulate in the focal ball in such quantity as to occasion a sensible rise.

Moreover, I can see no reason whatever why radiant caloric should not pass through water* as well as air. They are both fluids; they receive and transmit slow communicating caloric in a precisely similar way; namely, by a constant recession or migration of heated particles: they agree in many of their physical and chemical properties, such as great freedom of motion among their particles, extensive solvent power, elasticity, &c. What is there then I would ask in the constitution of water that should incapacitate it for the transmission of radiant caloric? Moreover, if not transmitted through this fluid, what then becomes of it? Is it converted into slow communicating caloric? This would be asserting their identity, a point about which I may remark philosophers are by no means agreed. But admitting the fact, it appears to me that such a conclusion would be rather favourable than otherwise to the hypothesis I have ventured to advance; for if slow communicating caloric be capable of *direct* transmission through fluids, and of this, the experiments of Hope, Murray, and Traill, permit us no longer to doubt, what difficulty is there in conceiving, that a peculiar modification of it should also be transmitted through the same medium.

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* There is no doubt that radiant heat can pass through water: the question is, whether it can commence its radiation *in* water.—*Ed.*

But it may be asked, allowing the validity of this theory to account for the deposition of the first stratum of ice, why does not each succeeding layer as fast as it is formed take that situation which its lesser specific gravity would assign to it, and rise to the surface of the water. This at first sight would appear a very formidable objection: it must, however, be recollected that this "ascensional" effort will only be exerted when the congealed mass is surrounded on every side by water. The aqueous crystals, as they may be termed, shooting from all the prominent points of the bottom, would, by their intertexture, become firmly infixed to the inequalities of the ground, and prevent the water from insinuating itself beneath." The continual deposit of sand and mud must likewise contribute to keep it sunk.

I am fully aware that the few remarks I have here ventured to offer on this curious and interesting subject require the support of actual experiment in order to give them the stability of a permanent theory. This, however, I leave to those whose leisure or abilities better qualify them for such pursuits. In the mean time they may supply the plan of more important information on the subject, and may, perhaps, be the means of turning the attention of chemists to the investigation of a phenomenon which has hitherto baffled all inquiry.

Article VII.

Meteorological Journal kept at Helston, Cornwall, for 1821. By Mr. M. P. Moyle.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Helston, Jan.* 20, 1822.

In sending the following meteorological journal kept at Helston, Cornwall, for the year 1821, for insertion in your *Annals*, it will be proper to state the circumstances under which it is formed. It consists of three observations daily, viz. at eight o'clock in the morning, at one at noon, and from ten to eleven o'clock at night, or as near as possible to those hours. The barometrical heights are very correctly made by a sliding index measuring from the surface of the mercury in the reservoir. The thermometer has a due northern aspect, and is insulated from the wall of the house, on which indeed the sun never shines. I give you only the mean of the three daily observations, stating at the foot of each month the maximum and minimum for that month. Where the wind was variable, the most prevailing for the 24 hours is given.

I am, Sir, your obedient servant, M. P. Moyle.

1821.	Barometer.	Therm.	Wind.	1821.	Barometer.	Therm.	Wind.	1821.	Barometer.	Therm.	Wind.
Jan. 1	29·756	28·6	NNE	Feb. 1	30·273	49·3	SW	Mar. 1	29·166	47·0	W
2	29·256	33·3	NNE	2	30·270	58·1	N by E	2	25 580	49·6	SW
3	29·066	29·66	E by N	3	30·250	43·7	SW	3	29·566	53·6	S by W
4	29·163	33·3	E by N	4	30·146	44·0	NW	4	29·646	51·0	SW
5	28·890	34·6	E by N	5	30·580	39·5	SW	5	29·800	52·0	S by W
6	28·940	40·5	E by S	6	30·583	44·5	S	6	29·513	51·6	SW
7	28·950	46·4	E	7	30·506	45·0	S	7	29·396	50·6	SW
8	28·906	43·6	SE	8	30·346	46·6	S	8	29·363	51·3	SW
9	28·873	43·6	S by E	9	30·090	37·3	N	9	29·490	52·0	SW
10	28·920	46·3	W by S	10	30·180	42·3	E	10	29·500	52·0	W
11	28·953	49·4	SW	11	30·176	40·6	E	11	29·633	44·5	SW
12	29·233	50·7	S	12	30·140	40·0	NE	12	29·916	50·0	SW
13	29·223	50·1	NW	13	30·203	43·0	E	13	29·950	48·6	SW
14	29·819	48·8	S	14	30·230	41·3	E	14	30·103	46·3	E
15	29·750	50·6	WNW	15	30·320	38·0	E	15	30·186	45·6	SE
16	29·960	49·3	SSW	16	30·326	36·5	E	16	30·060	48·6	E
17	30 046	50·3	S	17	30·196	42·0	E	17	29·843	49·6	E
18	30·173	51·0	S by W	18	30·163	38·6	NE	18	29·696	47·0	NW
19	30·143	51·0	S	19	30·266	39·6	E	19	29·510	44·4	NW
20	30·273	50·6	E	20	30·235	39·0	N	20	29·496	46·2	NW
21	30·480	47·6	S	21	30·195	43·0	E	21	29·403	46·6	N
22	30·520	48·6	E	22	30·220	42·6	E	22	29·850	44·2	N
23	30·563	47·3	E	23	30·183	40·0	E	23	29·945	44·4	NE
24	30·550	43·3	E	24	29·940	40·0	E	24	29·450	48·6	SW
25	30·490	45·6	E	25	29·860	42·6	E	25	29·463	47·0	NW
26	30·413	43·0	SE	26	29·836	43·0	E	26	29·273	44·3	S
27	30·230	42·3	S by E	27	29·433	41·6	E	27	29·376	45·0	NW
28	30·063	41·0	S	28	29·093	47·0	W	28	29·883	47·3	W
29	30·056	48·6	S by W					29	29·286	44·6	NW
30	30·220	49·6	SW					30	29·606	47·0	W
31	30·290	50·0	W					31	29·453	47·6	NW
Mean..	29·745	44·8		Mean..	30·152	42·4		Mean..	29·625	48·3	
Max..	30·60	54·0		Max..	30·62	52·0		Max..	30·20		
Min...	28·30	28·2		Min...	29·07	34·0		Min...	28·95		

29 days cloudy, rain, or snow.; 2 days fine, with occult clouds.

15 days cloudy, frost, and rain; 13 days fine with occult clouds.

18 days rainy, very cloudy, and hail showers; 13 days fine, with occult clouds.

1821.	Barometer.	Therm.	Wind.
April 1	29·483	47·00	S
2	29·383	37·66	WSW
3	29·410	48·33	W
4	29·393	46·00	NW
5	29·763	45·00	N
6	30·053	49·66	W
7	30·113	52·33	NW
8	30·133	47·40	N by W
9	29·973	52·56	SW
10	29·720	50·70	SW
11	29·503	50·00	SW
12	29·376	47·10	W
13	29·580	46·56	W
14	29·356	46·33	SW
15	29·476	45·60	W
16	29·376	45·33	W
17	29·543	46·66	W
18	29·713	52·33	W
19	29·390	51·33	SW
20	29·460	51·33	E
21	29·760	53·00	N
22	29·673	56·23	E
23	29·296	56·76	NE
24	29·340	56·32	SE
25	29·516	54·67	S
26	29·616	58·00	NE
27	29·686	55·00	N
28	29·793	55·60	NW
29	29·856	54·03	N by W
30	29·993	54·33	NW
Mean..	29·624	50·44	
Max ..	30·15	65·00	
Min...	29·23	37·00	

12 days rain and showers.

1821.	Barometer.	Therm.	Wind.
May 1	29·940	55·00	NW
2	29·790	54·33	SW
3	29·580	63·66	SE
4	29·582	53·33	S
5	29·400	51·66	S
6	29·442	45·00	SW
7	29·450	53·33	SW
8	29·744	59·60	W
9	29·956	52·68	W
10	30·201	56·06	SW
11	30·202	53·66	NW
12	30·080	52·60	W
13	29·866	49·00	W
14	29·460	50·60	W
15	29·350	50·00	W
16	29·446	48·34	W by S
17	29·820	51·65	SW
18	29·776	51·33	N
19	30·050	59·33	SSE
20	30·120	56·33	E
21	30·053	54·34	E
22	29·930	53·02	E
23	29·756	49·66	NE
24	29·756	48·66	NE
25	30·030	52·60	W
26	29·913	45·34	NW
27	29·876	47·68	NW
28	29·980	49·66	NNW
29	30·060	52·00	SW
30	30·093	54·34	ENE
31	30·000	53·65	SE
Mean..	29·840	52·36	
Max ..	30·260	60·00	
Min...	29·210	43·00	

13 days rain and slight showers.

1821.	Barometer.	Therm.	Wind.
June 1	30·000	54·33	WSW
2	29·923	58·33	SW
3	29·756	60·00	SW
4	29·600	57·33	SW
5	29·790	63·00	SW
6	29·896	55·55	SW
7	29·666	59·66	SW
8	29·703	57·33	N by W
9	29·863	51·66	NNW
10	29·823	51·33	NW
11	29·960	53·33	NE
12	30·260	55·00	NE
13	30·210	57·00	NE
14	30·250	56·00	SW
15	30·246	61·33	E
16	30·193	62·00	NE
17	30·230	62·00	E
18	30·236	59·00	E
19	30·113	58·33	E
20	30·050	59·34	E
21	30·080	60·00	NE
22	30·140	61·00	E
23	30·143	60·65	NE
24	30·066	60·00	E
25	30·060	61·33	E
26	30·060	61·66	E
27	30·046	60·33	E
28	30·130	60·66	SE
29	30·146	63·66	SE
30	29·777	61·00	S
Mean..	30·008	58·71	
Max ..	30·270	72·00	
Min...	29·600	47·00	

3 days slight rain.

1821.	Barometer.	Therm.	Wind.	1821.	Barometer.	Therm.	Wind.	1821.	Barometer.	Therm.	Wind.
July 1	29·616	63·00	SW	Aug. 1	29·936	63·00	NW	Sept. 1	29·960	66·0	NW
2	29·613	66·00	SW	2	30·046	66·00	SW	2	29·943	70·0	S by W
3	29·753	59·66	NE	3	30·030	67·62	W by S	3	29·806	69·6	S by W
4	30·106	56·00	NW	4	29·946	69·04	E	4	29·690	67·0	SSW
5	30·160	57·00	W	*5	29·833	67·60	W	5	29·810	66·0	SW
6	29·900	63·66	NW	6	29·970	61·66	W by N	†6	29·700	67·3	S
7	29·913	64·33	N	7	30·046	64·06	W	7	29·540	66·6	SW
8	30·053	63·66	N	8	29·736	61·00	W by S	8	29·620	63·2	W by N
9	30·163	59·66	NW	9	29·600	60·66	W	9	29·536	62·7	W
10	30·153	59·00	N	10	29·590	59·00	W	10	29·790	59·9	NW
11	30·063	66·66	E	11	29·733	61·30	W	11	29·910	63·5	SW
12	29·946	64·66	E	12	29·900	61·30	W by S	12	29·806	60·2	SW
13	29·876	63·66	E	13	29·920	62·68	NW	13	30·040	59·5	WNW
14	29·816	65·10	NW	14	29·836	62·66	W	14	30·073	63·7	NNW
15	29·796	60·20	W	15	30·033	62·02	N by W	15	30·176	61·3	N by W
16	30·043	62·46	NW	16	30·056	61·33	W	16	30·217	62·0	NW
17	30·106	67·23	S by W	17	30·046	64·66	W	17	30·172	64·3	NW
18	30·133	63·00	E	18	30·053	64·30	NW by N	18	30·056	63·0	W by N
19	29·866	67·00	S by E	19	30·100	67·66	E	19	30·013	61·6	NNW
20	29·793	65·00	SW	20	30·060	65·66	E	20	29·866	61·6	W
21	29·720	64·33	W	21	30.043	67·30	E	21	29·653	58·0	N by W
22	29·520	65·43	W	22	30·030	70·86	E	22	29·726	62·6	NW
23	29·573	61·23	W	23	29·910	68·80	W by S	23	29·653	61·0	W
24	29·536	59·33	SW	24	29·830	69·82	W	24	29·786	57·4	NW
25	29·790	66·00	W	25	29·900	65·43	SW	25	29·963	62·8	W
26	29·956	64·66	W	26	29·946	68·33	SE	26	29·906	63·3	SW
27	30·000	60·00	W by N	27	29·913	64·00	E by S	27	29·906	59·0	W
28	29·946	58·33	NW	28	29·670	67·00	S	28	29·603	60·3	SW
29	30·026	61·66	W	29	29·506	66·66	SW	29	29·796	55·3	NW
30	29·906	68·00	W	30	29·643	67·60	W	30	29·923	59·3	W
31	29·923	66·00	SW	31	29·850	65·30	W by N				
Mean..	29·893	62·77		Mean..	29·911	64·90		Mean..	29·856	62·4	
Max ..	30·220	78·00		Max ..	30·100	76·00		Max ..	30·220	76·0	
Min...	29·450	53·00		Min...	29·580	55·00		Min...	29·510	52·0	

12 days rain and slight showers.

14 days rain, slight showers, and foggy.

16 days rain and heavy showers.

* Thermometer in the sun, 95·00.

† Thunder, lightning, and very heavy showers.

1821.	Barometer.	Therm.	Wind.	1821.	Barometer.	Therm.	Wind.	1821.	Barometer.	Therm.	Wind.
Oct. 1	29·970	57·60	NW	Nov. 1	29·923	59·6	W	Dec. 1	29·760	51·0	W N
2	30·110	60·0	SW	2	29·900	60·0	SW	2	29·903	51·6	NW
3	29·800	60·6	SW	3	29·666	57·0	SW	3	29·690	49·3	NW
4	29·420	62·4	SW	4	29·720	57·0	NW	4	29·926	51·3	W
5	29·960	57·2	WNW	5	29·206	43·0	N by W	5	29·930	52·3	W
6	30·030	60·3	W	6	29·143	49·3	SE	6	30·006	52·3	E by S
7	29·943	61·0	SSW	7	29·920	52·3	SSE	7	29·903	54·3	SSW
8	30·060	53·6	W by N	8	29·850	55·3	SE	8	30·010	55·3	SW
9	30·043	56·2	S	9	29·890	56·0	SE	9	29·943	56·3	SSW
10	29·733	60·2	SE	10	29·860	57·3	SW	10	29·840	54·3	SSW
11	29·523	57·2	NW	11	29·870	55·6	SW	11	30·130	42·6	Variable
12	29·910	55·0	N by E	12	29·680	53·6	W by S	12	29·880	56·0	SW
13	30·230	55·3	E by S	13	29·846	56·0	S	13	29·786	52·3	W
14	30·230	57·3	SW	14	29·630	57·0	S	14	29·673	55·3	S
15	30·240	51·6	N	15	29·640	58·3	S	15	29·656	56·3	S
16	30·180	51·0	N	16	29·420	57·0	S	16	29·490	53·0	SW
17	30·050	53·3	N	17	29·286	54·0	SSE	17	29·100	52·0	SW
18	29·903	57·6	NW	18	29·490	53·3	S by W	18	28·933	52·0	SW
19	29·736	57 9	W	19	29·823	53·0	W by S	19	29·146	46·3	W
20	29·110	52·0	W	20	29 930	54·3	W by S	20	29·183	47·3	W
21	29 040	48·3	SW	21	29·796	50·0	W	21	29·326	51·0	W by N
22	29 120	53·6	W	22	29·776	56·0	W	22	29·223	52·0	W
23	29·210	54·0	W	23	29·716	51·0	W by N	23	29·416	48·0	W
24	29·626	54·6	W	24	29·176	53 3	SW	24	28·296	41·3	E
25	29·943	57·0	W	25	29·653	54·6	SW	25	28·316	41·0	W
26	29·986	59·3	S	26	29·743	54·3	S	26	28·455	40·2	NW
27	30·046	52·6	S	27	29·753	43·0	NNE	27	28·786	46·4	W
28	30 120	54·0	SE	28	29·740	54·0	W	*28	27 773	45·7	SW
29	30·080	58·3	SE	29	29·753	55·3	W	29	28·723	45·5	NW
30	29·923	57·0	S	30	29·796	52·6	W	30	29·333	47·0	NW
31	29·876	59·0	W					31	30·000	46·0	NW
Mean..	29·840	56·2		Mean..	29·796	53·8		Mean..	28·762	46·64	
Max..	30·25	63·0		Max..	30·23	61·0		Max..	30·16	58·0	
Min...	29·00	43·0		Min...	29·15	36·0		Min...	27.62	37·0	

14 days rain, fog, and showery thunder storm.

16 days rain and showery. Much thunder, &c.

21 days heavy rain, showers, and hail storms.

* On this day and previously, heavy gales.

Article VIII.

On the Presence and Proportion of Cadmium *in the Metallic Sheet Zinc of Commerce.* By E. D. Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, &c.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cambridge, Feb.* 6, 1822.

The phænomena exhibited by burning metallic *zinc* upon a disk of *platinum*, having excited in my mind a suspicion of the presence of *cadmium* in the *zinc* used for the experiment, I conceived that nothing would be easier than to have this matter put beyond doubt by a regular chemical examination of the *zinc* itself. I might, however, have spared myself some trouble if I had known, or rather had recollected, at the time that Professor *Stromeyer*, in the account published of his own experiments, mentions the fact of the presence of *cadmium* in *metallic zinc.** This circumstance was so little heeded by other chemists, to whom I had communicated my reasons for believing *zinc* contained *cadmium*, that had I not accidentally referred to the publication now cited, I might have continued in the belief that this circumstance had not hitherto been ascertained. It may, perhaps, however, appear to your chemical readers that the pains I have taken upon this subject have not been altogether nugatory, if I shall succeed, as I hope to do, in making them acquainted with some properties of *cadmium* which either were not before observed, or respecting which the accounts before published were in themselves erroneous. First, then, in the precipitation of metallic bodies by *iron*, previous to the examination of a salt containing *cadmium*, it may be stated, as a doubtful point, whether, if the action of the *iron* be continued long enough, some, if not all, of the *cadmium* may not be precipitated. I have the greater reason to rely upon an experiment which I made with a view to ascertain this point, because I used for the preparation of the salt of *cadmium*, some *oxide of cadmium* from Professor *Stromeyer* himself, which *Dr. Wollaston* had kindly presented to me. Having dissolved this oxide in *muriatic acid*, and neutralized the solution by evaporation with a very gentle heat, and the addition of distilled water, I suffered two *cylinders* of polished *iron* to remain in the liquid during 24 hours. Previously to the placing of the *iron* in the liquid, it yielded an orange-yellow precipitate to *sulphuretted hydrogen;* and a white precipitate to *carbonate of ammonia*, which had all the characters of the *carbonate of cadmium*. But no change of colour was caused by *sulphuretted hydrogen* after the *iron* had been immersed for the time specified; nor could I any longer obtain a satisfactory proof of the presence of *cadmium* by the usual tests.

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* See *Annals of Philosophy*, vol. xiii. p. 108. 1819.

The account which appeared in a volume of the *Annals of Philosophy*,* published in *October*, 1819, with the title of *New Details respecting Cadmium, by M. Stromeyer*, taken from the *Annalen der Physik*, lx. 193, mentions as a property of *cadmium*, that "the precipitate formed by the *carbonate of ammonia* is not soluble in an excess of this solution, *zinc* exhibiting a different property." This is certainly erroneous, if Prof. *Stromeyer's* own *oxide of cadmium* may be considered a proper substance for preparing the salt necessary for the experiment. Having dissolved a portion of this oxide in pure *muriatic acid*, and neutralized the solution, as before, adding distilled water, *carbonate of ammonia* yielded a white precipitate, which was wholly soluble in an excess of the *carbonate*. The importance of attending to this fact will be evident to all your readers who pursue the process pointed out by Prof. *Stromeyer* for obtaining *cadmium* from its ores. I think it also right to mention (with a view of putting chemists upon their guard, who have not had more experience in these matters than myself), that some of the phænomena exhibited by the combustion of *cadmium*, so nearly resemble those exhibited by *lead* under the same circumstances, that the absence of the last mentioned metal ought always to be carefully ascertained. Before I proceed, therefore, to relate an account of experiments which have enabled me to separate *cadmium* from *zinc*, it will be proper to mention such characters of the former metal as may serve to identify it under all circumstances. For this purpose, owing to the brevity and perspicuity with which the present Regius Professor of Chemistry at *Glasgow* has pointed out these properties, I shall quote two letters I had the honour to receive from him upon this subject, when he kindly undertook to examine some *carbonate of cadmium* which I had obtained from an English ore of *zinc*, and when he confirmed by his own observations the fact of the presence of *cadmium* in the mineral I had examined. According to *Dr. Thomson*,† there are certain trials which may be considered as affording the "*experimentum crucis*" with regard to this body, especially in distinguishing CADMIUM from ZINC. (1.) "*Phosphate of soda* precipitates *zinc* in small crystalline scales; it precipitates *cadmium* in a white pulverulent powder.‡ (2.) *Sulphuretted hydrogen* throws down ZINC *white*, but CADMIUM *yellow*, which remains fixed at a red heat." Other chemical characters of *Cadmium* are, the solution, with effervescence, both of its

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* See vol. xiv. p. 271.

† MS. Letters of *Dr. Thomson*, dated *Glasgow*, Jan. 25, 1820, and Feb. 1, of the same year.

‡ It also precipitates *lead* in the same form, but *lead* precipitated by *carbonate of ammonia* is not redissolved by adding an excess of the *carbonate*, or by liquid *ammonia*.

oxide and *carbonate*, and of *metallic Cadmium* in *muriatic acid*. The oxide obtained from this solution by heat and alkalies has a yellowish-brown colour. The *muriate*, as before mentioned, is precipitated *white* by *carbonate of ammonia*, and is redissolved by adding an excess of that *carbonate*. It is moreover precipitated white by *potass*,* *ammonia*, and *sulphate of soda*; yellow by *sulphuretted hydrogen*, and white by *prussiate of potass*.

To these characters may now be added the striking phenomena observed in the combustion of CADMIUM, by *Dr. Wollaston*, and by *Berzelius*.

According to *Dr. Wollaston* the *oxide of cadmium* is *white*; when fixed at the point of the *blue flame*, before the blowpipe, supported upon a *platinum* disk, the *carbonaceous* matter of the flame reduces the oxide, so that the metal, being revived, burns, and deposits during its combustion, a *protoxide of cadmium*, of a *reddish-brown*, or *copper colour*, easily to be recognized by those who have once seen it. According to *Berzelius*, whose observations in point of time succeeded those of *Dr. Wollaston*, substances containing *Cadmium*, when exposed to the action of the blowpipe, and supported upon *charcoal*, yield a *yellow oxide*, which is deposited in the form of "a ring" around the body exposed to trial. He calls it "*un anneau jaune, ou orangé, d'oxide de cadmium*;" and such, he maintains, is the subtlety of this test, that the *carbonates of zinc* which do not contain more than one per cent. of the *carbonate of cadmium*, exhibit this appearance; insomuch, he adds, that if there be no manifestation of this "*anneau jaune*," it is a proof that the substance under examination does not contain *Cadmium*.

Your readers will thus be put in the possession of a few facts, within a small compass, which will be found useful in judging of the validity of the following observations.

Having exposed some filings of *zinc* upon a *platinum* disk before the *blue flame* of a wax candle, urged by the common blowpipe, I perceived that the polished surface of the *platinum* was altered by the experiment, and that an appearance resembling that of the *protoxide of cadmium*, produced under similar circumstances, was apparent upon the metal. To see whether this appearance was owing to the *lead* which is contained in the *metallic zinc* of commerce, I exposed some *white oxide of lead* to the same trial, and obtained nearly a similar result; but with marks of a fine blue colour mixed with hues of yellow and of reddish-brown. I, therefore, resolved to submit the *zinc* to a chemical examination. For this purpose having dissolved it in *muriatic acid*, and neutralized the solution, adding distilled water, I suffered *iron* to precipitate as many of the metallic impurities as that metal would throw down during some hours that it remained immersed in the liquid, which was then filtered, and received into a *platinum* capsule containing a piece of clean *zinc*. In a short time both the interior of the *platinum* capsule and the surface of the *zinc* were coated over with a precipitate of a dull leaden hue; and this, being washed, exhibited before the blowpipe, and also after solution in *muriatic acid*, all the characters before mentioned as peculiar to *cadmium*, with this exception, that the precipitate yielded by the *muriate* to *sulphuretted hydrogen* was somewhat darker than the precipitate caused when *cadmium* is precipitated by the same reagent, which made me suspect that it was still contaminated with *lead*. I, therefore, went to work in another way, and dissolved the *zinc* in dilute *sulphuric acid*, following *Stromeyer's* process when obtaining *cadmium* from the *sulphurets of zinc*, which contain *sulphuret of lead*. As soon as all the *zinc* had been dissolved, I took care to have a great excess of acid present in the solution by adding fresh *sulphuric acid* to the liquid, which was afterwards filtered. I then sent a stream of *sulphuretted hydrogen* gas through it, which in the space of a few minutes communicated to the solution the fine *orange-yellow colour*, which characterizes the precipitation of *cadmium* by means of that reagent; but many hours elapsed before the precipitate was sufficiently disengaged to subside. As soon as it had subsided, it was of dingy *yellow* colour. The supernatant fluid being then decanted, *muriatic acid* was poured upon the precipitate, and slowly evaporated. Afterwards distilled water being added to the dry *muriate*, the liquid was filtered, and it exhibits the following properties:

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* Liquid *caustic potass* precipitates *cadmium* white, in the form of a *hydrate*, which is not redissolved by an excess of the precipitant; and this distinguishes it from zinc.

1. *Carbonate of ammonia* causes a *white* precipitate which, by excess of the *carbonate*, is redissolved. The solution evaporated to dryness, and the residue exposed to a smart red heat in a porcelain crucible, affords an *oxide*,* similar to that mentioned by *Stromeyer*. He says *Cadmium* forms only a single *oxide*, 100 parts of the metal combining with 14·352 of oxygen. "The colour of this oxide varies according to the circumstances in which it is formed. It is brownish-yellow, light-brown, dark-brown, and even blackish. It is quite fixed, and infusible in the strongest white heat, and does not lose its oxygen."†

The oxide I obtained by exposing the *carbonate* to a violent heat agrees with *Stromeyer's oxide*; but, in one instance, instead of turning to the brown colour of snuff, which that does, it remained scarcely altered by heat. In this respect, it could not be considered as agreeing with *Stromeyer's* own

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* This would, perhaps, afford the finest yellow pigment known; and when it is considered that a very powerful temperature is necessary to produce it, perhaps it is of all colours the least likely to be affected by atmospherical changes of temperature afterwards; neither would it be blackened by exhalations from the coal fires of our apartments; but the colour is rarely in two instances alike; it is sometimes of a fine orpiment-yellow, and at others of a darker hue.

† See *Annals of Philosophy*, vol. xiv. p. 270. 1819.

oxide of Cadmium, with which, however, in other chemical properties, it remarkably corresponded.

In subsequent trials I obtained a yellowish oxide, similar in colour and in chemical characters to that which I have from *Prof. Stromeyer.* It changes to a *snuff-brown* and even *black* colour when exposed to heat before the blowpipe, and regains its own colour afterwards when cold. Five hundred grains of *zinc* yielded exactly a single grain of this oxide by the process I have described; so that allowing, according to *Stromeyer,* that 100 parts of the oxide of cadmium contain 14·352 of oxygen, the proportion of metallic cadmium in 2-10ths of a grain of the oxide (which is all I had obtained from 100 grains of zinc) would equal $\frac{17}{100}$ of a grain nearly, and all the *cadmium* which, in the *metallic* state, is contained in 10,000 pounds weight of *metallic zinc* is nearly equal to 17 pounds. The *cadmium,* therefore, in this alloy exists nearly in the state of the pure *gold* in our last *silver* coinage, and in very small quantity. But to proceed with the other chemical qualities of the liquid I have mentioned:

2. *Phosphate of soda* causes a *white* pulverulent precipitate, which is redissolved by adding liquid *ammonia.*

3. *Liquid caustic potass* causes a *white* precipitate which is not soluble by adding the *potass* in excess.

4. *Zinc* immersed in the solution becomes invested by a precipitate of a leaden hue, which, after solution in *muriatic acid,* exhibits the characters of *metallic cadmium.*

5. *Sulphuretted hydrogen* causes a precipitate which is at first of an *orange-yellow,* and afterwards of a dingy *yellow* colour.

6. All these precipitates, when exposed to the action of the blowpipe upon *platinum* or *charcoal,* have the habitudes of CADMIUM. But if any chemist shall hereafter be able to prove that a substance may possess all the characters I have enumerated, and yet, after all, not be *Cadmium,* no one will be more thankful for the intelligence than your humble servant.

I remain, dear Sir, &c.

EDWARD DANIEL CLARKE.

ARTICLE IX.

Memoir on a Deposit found in the Waters at Lucca. By Sir Humphry Davy, Bart. President of the Royal Society, London, and Member of the Royal Academy of Sciences, Naples.*

THE waters of the baths at Lucca, at the spot where the temperature is the greatest; that is to say, in what are termed the caldi or hot baths, eject in a considerable quantity a substance that produces a deposit of a brownish-yellow hue. Having collected various quantities of this deposit, and having submitted it to chemical experiments, I have discovered it to be a compound of oxide of iron and silica: not having a balance sufficiently accurate, it was impossible for me to ascertain with precision the exact proportions: in the single experiment, however, that I made for this purpose, the oxide of iron was to the silica in the proportion of 4 to 3.

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* From the Memoirs of the Royal Academy of Sciences at Naples.

It is extremely probable that the oxide of iron and the silica had been dissolved together in the water, and deposited at the same time, because the silica being separated from the oxide by means of a weak acid, it appears to resemble gelatine, and because the deposit, when examined in its natural state, was found to be uniform in its substance, even when looked at through a lens.

Although the oxide of iron, when first discovered, proves to be peroxide, it is nevertheless very probable that it exists in the water in the form of protoxide, or that it is converted into peroxide by the action of the air which is dissolved in the water. The probability of this opinion is further confirmed by the circumstance, that the colour of the water is not changed by the addition of the triple prussiate of iron, nor by that of gallic acid, it being well known that protoxides generally have a greater disposition than peroxides.

The analogy which I established some time since, during my researches as to the decomposition of alkalies and earths, between the base of silica and that of boracic acid, and the facts described by MM. Smithson and Berzelius, furnish reasons for classing silica among the acids; and it seems probable that the oxide of iron and the silica undergo a real chemical combination in the warm water, and that they separate from it in consequence of its cooling after issuing from the mountain.

When the deposit is obtained from its diffused state in water, it contains no other substances than oxide of iron and silica; when it is taken from the bottom of the waters, carbonate of lime and sand may be observed mixed with it. These two substances are, however, evidently extraneous. From many experiments which I made I am convinced that after it has quitted its source, the water yields no deposit whatever; but it appears certain that the water, which, on rising from the spring, possesses a temperature of 112°, must be much warmer within the mountain, and that consequently its solvent power must there be much greater.

When a considerable quantity of it is evaporated, a small portion of silica and oxide of iron is found, a discovery that had been made by Signor Battista Tessandori; and I have ascertained by experiments that these substances are obtained in the same state, and nearly in the same quantity, in which I have stated them to be discovered in the brownish-yellow deposit.

A small portion of oxide of iron is found in the Bath Waters, where likewise it is accompanied with silica; nor is it improbable that this earth is in many cases the cause of the oxide of iron being dissolved in the water; and these facts combined furnish us probably with an explanation of the manner in which ochre is generated. As to what may be the effect of the combination of oxide of iron and silica on animal bodies, it is the province of medical men to examine, and to determine upon, after long and adequate experiments.

Article X.

On the Formation of Carburet of Nickel, and Method of obtaining the Metal pure. By Mr. Nicholas Mill.

(To the Editor of the *Annals of Philosophy.*)

SIR, *London, Nov.* 16, 1821.

Little being known relative to nickel and its compounds, particularly of the formation of the carburet which was first glanced at by Dr. Thomson in a paper on the purification of this metal, inserted in the *Annals of Philosophy*, the originality of which was lately laid claim to by two individuals in two late papers in the same publication, I have taken the liberty of forwarding to you the following process:

Let the native arseniuret or sulphuret of nickel be finely pounded and mixed with charcoal also pounded, and placed in a flat bottomed crucible, and exposed to a dull red heat for two hours. Blow off the charcoal with a pair of bellows, and dissolve the nickel in nitrosulphuric acid. Evaporate and crystallize. Beautiful green crystals of the form of a square solid will be obtained. Let these be carefully selected, dissolved in water, recrystallized, and mixed with a small portion of borax and pounded charcoal, and fused for a quarter of an hour in a strong red heat. When cold, break the crucible, and underneath the borax will be found a button with a very high degree of lustre, very fusible and magnetic, which latter fact proves its freedom from arsenic,* from which substance, by other processes, it is very difficult to free it. The carburet thus formed is brittle and fusible, and if exposed to the flame of the oxyhydrogen blowpipe becomes malleable; but it may still, and most commonly does, contain copper and iron. In order, therefore, to obtain the malleable metal pure, dissolve the carburet in nitric acid, neutralize the solution, and precipitate by aqueous potash. Redissolve the precipitate in aqueous ammonia, saturate the excess of alcali by nitric acid, and precipitate the copper and iron by a bar of zinc. Nothing will now remain in solution but nickel, and, perhaps, a little cobalt. To obtain the nickel free from this metal, precipitate it by aqueous potash, and the cobalt will remain in solution. If this last precipitate be dried and mixed with borax and exposed to a heat of 160° of Wedgwood, the metal will be reduced in a pure state.

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* Chenevix; Richter.

I am, Sir, your obedient servant,

NICHOLAS MILL.

ARTICLE XI.

Chemical Examination of Cubebs. By M. Vauquelin.*

CUBEBS are the fruit of the piper cubeba (triandria trigynia), a perennial plant which grows in the Philippine Islands, in Java, Guinea, and the Isle of France.

The cubebs which were employed for these experiments were presumed to have been collected three years; they were imported by the Dutch, and had been in a warehouse in Paris for 18 months.

These grains do not all possess the same colour; they are rounded, and attached to a stalk: if they are examined after being macerated in water, four coverings are discoverable; the first is fleshy, and softens in water; the second is of a grey colour, and is nearly transparent; the third is thin like the peel of an onion, of a yellowish-brown colour; the fourth is a very thin white pellicle immediately covering the grains, some of which are round, and entirely fill their covering; others are flat on one side, and rounded on the other; others again are wrinkled and covered with fatty matter; and lastly, there are some which are of a white colour.

Many of these grains contained a white concrete oily matter, and which had every appearance of a crystal. This concrete matter melted in a silver spoon, remained fused, and without emitting any odour.

Seven hundred and seventy-two grains of bruised cubebs were put into a retort with water, and subjected to distillation. The water which came over was turbid, and covered with small drops of volatile oil, the consistence of which was greater than that of common volatile oils. It had a strong taste, which somewhat resembled that of peppermint. In this respect, Murray has committed a great error: he says that Beaumé obtained two ounces and one drachm of this oil from two pounds and a half of cubebs; whereas it was only one ounce one drachm from twelve pounds and a half of that grain which Beaumé obtained.

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* From the Memoires du Museum d'Histoire Naturelle, tom. vi.

The distilled water possessed all the odorous properties of the oil, but it was alkaline, for it restored the blue colour of litmus paper which had been reddened by an acid; wishing to know from what alkali this property was derived, I saturated it with weak sulphuric acid, and evaporated the solution.

The residue of the evaporation being examined contained a salt, that had all the characters of sulphate of ammonia mixed with a little essential oil. It emitted ammonia upon the addition of a few drops of potash. When put on a hot iron, it was volatilized in the form of dense white vapours.

The residue of the distillation when filtered had a brown colour, a bitter taste, and with reagents the following effects were produced:

1. It gave an abundant yellow flocculent precipitate with nitrate of silver, a great part of which was redissolved by pure nitric acid.
2. Oxalate of ammonia produced no effect.
3. Nitrate of barytes gave a light flocculent precipitate.
4. Galls gave a bulky brown precipitate.
5. Acetate of lead gave an abundant precipitate, and the liquor was rendered almost colourless.
6. It reddened litmus paper slightly.

By evaporation, it gave a brown extract, which was slightly acid. It was divided into parts, and subjected to several operations.

1. It was treated with alcohol, to which it imparted a yellow colour: the alcohol, when evaporated, left a substance, of a fine colour, which redissolved in water, except some particles of a brown dry resin, that softened between the teeth, and had an acrid taste. That part of the extract which was insoluble in alcohol was dissolved in water, but there remained some particles in the liquor which appeared to be albumen coagulated by the alcohol.

2. A portion of this extract being burnt gave an ash containing subcarbonate, phosphate, a little muriate of potash, and some phosphate of magnesia.

3. The remaining part of the extract was dissolved in water, and precipitated by acetate of lead. The precipitate being washed and decomposed by sulphuretted hydrogen, gave a small quantity of malic acid mixed with a little colouring matter; the liquor from which I had separated this acid by means of acetate of lead, being treated with the subacetate of the same base, gave

a yellow precipitate which, being put into alcohol, was dissolved with the exception of a few particles, possessing all the properties of gum; the yellow precipitate assumed a rose colour, with a shade of violet, by the action of heated sulphuric acid.

The liquor from which the above-mentioned precipitates were obtained was in its turn exposed to a current of sulphuretted hydrogen gas. The lead being separated by these means from the liquor, there was obtained, by evaporating the latter, a yellowish matter of a nauseous odour, and of a flavour resembling that of raw peas, and the substance which is found in leguminous plants. It is precipitated by galls, and dissolves better in weak than in strong alcohol. When heated in a tube, it gives all the products common to vegetables, without any mixture of animal matter.

The different substances which have been described do not appear to be the active matter of the cubebs; the grains which had been acted upon by water were treated with boiling alcohol. This solution being filtered left upon evaporation a green substance which had peculiar properties, and the appearance of fatty matter. It is fluid, of a disagreeable smell, an acrid bitter taste, somewhat resembling that of balsam of copaiva, and it occasions slight irritation in the throat. When put upon paper, it occasions spots, like fat oils. This paper, when heated, gives out a little volatile oil, which was mixed with the fatty matter, but the paper remained spotted.

The fatty matter, when washed with water, communicated to it a little pungency; the water, when evaporated, left an extractive matter which had also been taken up; dissolved in cold ether, it left a residue of a resinous nature. This oil when obtained by means of ether from the grain separated from the shell is much whiter, because the shells contain a much greater quantity of colouring matter; the fatty matter was subjected to ebullition in weak sulphuric acid, in the hope of separating its acrid property; the plan did not succeed; I merely remarked that the sides of the vessel to which it was attached became coloured from a rose-red to a violet-purple: if water be poured upon this colouring matter, it changes in colour, and becomes blue. Balsam of copaiva, and even turpentine, treated by sulphuric acid, became of the same colour.

Wishing to know whether this substance existed in the shell, or only in the grain, I took 77·22 grains separated from all their shells, excepting the two last. This quantity digested in alcohol gave about 2¼ grains of the matter in question; while 77·22 grains of the shells which contained no kernel gave me scarcely one grain of it. It is, therefore, evident, that this matter occurs in every part of the grain, but most abundantly in the centre.

This peculiar matter dissolves in ether or in alcohol; submitted to distillation it yields a small quantity of essential oil, but the residuum is transparent, solid, and possesses an acrid taste; and is soluble in ether, alcohol, and potash, from which it is precipitated by an acid. When compared with balsam of copaiva, it bears some resemblance to it, but there are some points of difference. When it is procured by means of ether, its colour is like that of balsam of copaiva.

When these substances are put into distilled water, they impart a disagreeable taste to it; if these solutions be afterwards evaporated, extractive matters are obtained, which bear the strongest resemblance to each other, and when treated with sulphuric acid, assume a colour varying from rose-red to purplish-violet. The balsam of copaiva treated with this acid suffers the same changes, excepting that the colour produced by it is not so fine.

The matter from the cubebs dissolved in alcohol and submitted to distillation gives some volatile oil; the same happens with the balsam of copaiva, but the odour of the latter is more disagreeable than that of the former.

Ether has similar action upon these two substances: the matter of the balsam of copaiva when treated with carbonate of soda becomes of a fine white colour; the resin of the cubebs assumes a light yellow; the two solutions underwent different changes by ebullition, that of the balsam of copaiva did not afford any precipitate; the contrary occurred with the substance obtained from the cubebs.

Notwithstanding these slight differences, which may depend upon some colouring principle retained by the resin of the cubebs, there can be no hesitation in believing that a strong resemblance exists between it and the balsam of copaiva, and that it is this peculiar matter in which that property resides that has been discovered and employed in the cure of gonorrhoea.

Being desirous of discovering whether the kernel contained any essential oil, I separated the shells very perfectly, and submitted the kernels to distillation. I obtained by this process a distilled water similar to that which had been procured from the entire grain; some small drops of oil were apparent at the surface of the water; but the smallness of the quantity prevented me from making an exact comparison between it and that obtained from the entire cubebs.

About 150·5 grains of cubebs were burnt, and left an ash, weighing about one grain; it had a green colour, like that communicated to potash by manganese. Treated with water, this residuum lost nearly half a grain of soluble salts, composed of subcarbonate, phosphate, and a little muriate of potash. The residuum insoluble in water was taken up by muriatic acid, excepting a small quantity of silica: this solution was found upon examination to be composed of phosphate of magnesia, and a trace of iron and manganese.

The resinous matter of cubebs, decomposed in a glass tube, exhibited the usual appearance of vegetable matter; the vapours which were disengaged had a penetrating odour, and reddened litmus paper.

It is evident from this analysis that cubebs contain:

1. A volatile oil which is nearly solid.
2. Resin, resembling that of balsam copaiva.
3. A quantity of another and coloured resin.
4. A coloured gummy matter.
5. An extractive principle similar to that which is found in leguminous plants.
6. Some saline substances.

I am desirous that this analysis, upon which I have bestowed some pains, may serve to direct physicians to that employment of cubebs in the healing art, to which they may think them applicable.

Article XII.

On the Method of analyzing the Ores of Nickel, and on a new Combination of Nickel with Arsenic and Sulphur. By J. Berzelius.[*]

Within a few years, two new metals have been announced as discovered in the ores of nickel (vestium and wodanium). It was afterwards found that these metals were only alloys of nickel with iron and arsenic. Even the celebrated Richter was deceived 20 years since by a similar alloy, which he took for a new metal, and named niccolanum.

The cause of all these mistakes is to be attributed to the imperfection of the analytical methods which have been employed to separate nickel from other substances, especially from the arsenic and iron with which it is accompanied. Several metallic arseniates, especially that of iron, possess the property of dissolving in acids, as if they were weak salifiable bases; the alkalies precipitate them without altering their composition, and when these oxides are reduced by means of charcoal, arseniurets are obtained, which, when compared with the pure metals, appear to be peculiar metallic substances. Chemists who are accustomed to examine every thing by the blowpipe cannot, however, be deceived in this respect, because even the smallest trace of arsenic is detected by the smell, when these substances are heated with soda upon charcoal.

I. *Common Method of analyzing the Ore of Nickel.*

The powdered mineral is dissolved in nitric acid. There remains some sulphur mixed with a little silica. The residue is weighed, the sulphur is burnt, and the remaining silica is again weighed.

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[*] From the Annales de Chimie et de Physique, tom. xvii.

The nitric solution evaporated to dryness is again treated with concentrated nitric acid to acidify the arsenic, and render the oxide of iron insoluble,* which is afterwards separated. This latter process is nevertheless absolutely incorrect; for the arseniate of iron readily dissolves when the solution contains acid in excess.

The acid solution, nearly neutralized by an alkali, is precipitated by nitrate of lead, which separates arseniate of lead; but as this compound is soluble in nitric acid, it must be evaporated to dryness, and the dry mass treated with water. It is very true that if the iron could be separated by the method above described, we should succeed in separating the nickel from the arsenic by means of nitrate of lead. Usually, however, the precipitate thus obtained contains arseniate of iron and arseniate of lead, in the mixture of which it is impossible to calculate the quantity of arsenic. Added to this, when an arseniuret is dissolved in nitric acid without the addition of muriatic acid, a great quantity of arsenic is converted into arsenious acid, and the precipitate becomes a mixture of arseniate and arsenite of lead.

From the solution which contains the nickel and the lead added in excess, the latter is precipitated by sulphate of soda; afterwards sufficient ammonia is added to redissolve the oxide of nickel, and the alumina and every other substance mixed with the ore of nickel, which is insoluble in ammonia, is obtained. The ammoniacal solution is to be evaporated, and the nickel precipitated by subcarbonate of soda or potash, taking care to evaporate every trace of ammonia liberated by the carbonate.

The oxide of nickel, thus obtained, usually contains cobalt. M. Thenard and Fourcroy attempted to separate those oxides by peroxidizing them by means of oxymuriate of lime, and treating the peroxides with ammonia, which decomposes and dissolves the peroxide of nickel, but not the peroxide of cobalt. This method, however, is not quite correct; for the portion dissolved contains a little cobalt, and the residuum contains nickel.

Mr. Phillips discovered another method, and one which is more proper to be employed in analyses. It consists in diluting the ammoniacal solution of the two oxides with a considerable quantity of water, after which a solution of potash is to be added as long as precipitation takes place. The nickel is precipitated, and the cobalt remains in the liquor from which the ammonia is obtained by evaporation. This method is not rigorously exact, but the traces of cobalt which are precipitated with the nickel may be entirely neglected with respect to analytical result.

M. Laugier afterwards discovered another method of preparing nickel absolutely free from cobalt, by slowly evaporating an ammoniacal solution of the oxalates of nickel and of cobalt. The oxalate of nickel is deposited, and that of cobalt remains dissolved in the form of a double oxalate of cobalt and ammonia. But this process, as M. Laugier observes, cannot be employed in an analysis.

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* Aikin's Dictionary of Chemistry, ii. 136.

Dr. Thomson proposes, in order to obtain pure nickel, to dissolve the ore of this metal in a mixture of sulphuric and nitric acid, which leaves the greater part of the arsenious acid undissolved: to the filtered solution, potash is to be added, and then the double sulphate of nickel and potash is to be crystallized. By this method the arsenic is separated, for the crystals do not contain any; but if the ore contains cobalt, zinc, and copper, these metals also form double salts with potash which are not separable by crystallization from that of nickel. Added to this, Dr. Thomson's method is not applicable to analysis.

It is to M. Stromeyer that we are particularly indebted for a knowledge of the composition of nickel ores. It is he also who informed us that wodanium and vestium are not peculiar metals;* but he has not yet, as far as I know, described the analytic method by which he obtained his results. This circumstance is nevertheless extremely important; for without it, the probability of the result depends entirely on the confidence which is placed in the author.

M. Berthier has given us analyses of an arseniuret and of an arseniate of nickel. We owe to him a very good process for determining the quantity of arsenic acid in a solution deprived of insoluble bases. It consists in dissolving a given weight of iron in nitric acid; this solution is to be poured into the liquid from which the arsenic acid is to be separated, and precipitated by means of ammonia. The precipitate formed is subarseniate of iron, which after being heated is to be washed, and the quantity of arsenic acid is discovered, because that of the oxide of iron was previously known.

M. Psaff, of Kiel, has lately examined the same ore of nickel, which is the principal subject of this memoir. The observations which I have already made upon the ancient methods of analyzing these ores are for the most applicable to that selected by M. Psaff, and which I consider it useless now to describe. He found this ore to consist of

Nickel	24·42
Arsenic	45·90
Iron	10·46
Sulphur	12·36
Loss	6·86

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* That vestium is not a peculiar metal was also shown by Mr. Faraday in the Royal Institution Journal, vol. 6. p. 112, but whether before or after M. Stromeyer, I do not know.—*Ed.*

This ore of nickel excited my curiosity some time since, when I examined several minerals in order to discover selenium in them. The sulphur which it contains proved to me that it differs entirely from arsenical nickel, and I intended to analyze it upon a future occasion. In the mean time, M. Pfaff published an analysis of it, which might render any further examination superfluous, if there had not been so considerable a loss. Some phenomena which I observed at the time of my first experiments induced me to undertake this analysis; and the difficulty of obtaining a satisfactory result engaged me in numerous researches.

II. *Examination of some Substances which are obtained in the Analyses of the Ores of Nickel.*

Arseniate of Iron.—The red oxide of iron combined with the arsenious and arsenic acids is soluble in caustic ammonia, and gives a red coloured solution. If the solution also contains sulphuric and nitric acids, it deposits in a few days a yellow powder, which dissolves in water, in attempting to wash it upon a filter. If a solution of arseniate of peroxide of iron in nitric acid be evaporated until the greater part of the acid is volatilized, a white powder is obtained, that is insoluble in water, and which is neutral arseniate of peroxide of iron. When subjected to a scarcely visible heat, it loses 17·68 per cent. of water, and becomes red; but if the fire be increased to redness, it appears to ignite for a moment, and becomes yellowish-white. The water contains twice as much oxygen as the base. If caustic ammonia be poured upon this arseniate before it is dried, it is readily dissolved; the dry arseniate requires some digestion to dissolve it. The red solution left exposed, evaporates, and loses its excess of ammonia, but does not deposit any thing, and finishes by forming a transparent red mass. This mass is a double subarseniate of peroxide of iron and ammonia. When heated in a proper apparatus, it gives at first much ammonia and a little water; but at the moment in which the mass begins to redden, water, azotic gas, and arsenious acid, are disengaged, and the latter sublimes. The residuum acquires a greenish colour, and appears to be an *arsenias-ferroso-ferricus.* The double subsalt in question dissolves in water mixed with a little ammonia, but pure water decomposes it, and dissolves arseniate of ammonia, as well as a small quantity of undecomposed double subarseniate, the residue left being a subarseniate of peroxide of iron.

The subarseniate of iron is not soluble in ammonia, even though it be first dissolved by an acid, and ammonia added to the solution. Consequently when in a solution which contains peroxide of iron and arsenic acid, the latter is sufficient only to form a subarseniate of the peroxide, the addition of ammonia produces no trace of soluble double subarseniate; but each portion of arsenic acid that is added renders a certain quantity of the peroxide soluble in ammonia. The subarseniate of peroxide of iron which contains the slightest excess of base, and is insoluble in ammonia, is that which is formed by the oxidation of the neutral arseniate of the protoxide. Consequently, if neutral arseniate of protoxide be dissolved in nitromuriatic acid, and ammonia in excess be added to the solution, all the arsenic acid and all the peroxide of iron are precipitated.

Caustic potash even, when in great excess, does not completely decompose arseniate of peroxide of iron. I twice digested the same portion of this compound in strong solutions of caustic potash, which left a substance perfectly similar to peroxide of iron. When dried at the heat of boiling water, and afterwards heated to redness, it lost 0·134 of its weight, which was water; the remaining 0·866 was dissolved in muriatic acid, and precipitated by hydrosulphuret of ammonia.

The sulphuret of iron, after being well washed, was dissolved in nitric acid: precipitated by ammonia, it gave 0·796 of peroxide of iron. The arsenic acid, therefore, weighed 0·07. The oxide of iron contained 24·4 parts of oxygen; the water contained 12, and the arsenic acid 2·43 parts; consequently these quantities are to each other as 1, 5, and 10. This substance then, if it be not a mixture, is composed of subarseniate, with water of combination, and hydrate of peroxide of iron. When made slowly red hot, it suffers more rapid combustion than most other substances in which I have hitherto observed it.

The arseniate and arsenite of protoxide of iron are also soluble in ammonia, but less so than those of the peroxide: the solution when exposed to the air assumes a greenish colour.

The arseniate of nickel dissolves in ammonia in whatever proportion the arsenic acid is combined; but if in a solution of arseniate of nickel there is any peroxide of iron, and if the arsenic acid be not in sufficient quantity to form neutral salts with the two oxides; the ammonia precipitates not only some subarseniate of iron, but also some subarseniate of nickel, in the form of a double subsalt, which is of a green colour. If the arsenic acid is not sufficient to form the double subsalt, it forms a mixture of subarseniate of iron with the double salt, and in this case, the precipitate has more or less the colour of oxide of iron.

If the arseniate of nickel does not contain any oxide of iron (protoxide or peroxide), it is totally decomposed by caustic potash, especially if it be first dissolved in ammonia, and the solution of potash be afterwards poured in. A compound of nickel and potash precipitates, and the arsenic acid remains in solution combined with the alkalies. If, on the contrary, the liquid contains iron, the precipitate contains arsenic.

A solution of arseniate of iron and nickel, saturated with an alkali even to the commencement of precipitation, and afterwards mixed with a solution of acetate of lead, precipitates arseniate of lead and arseniate of iron, until no more remains in the liquid. This circumstance arises from the affinity of the arsenic acid being so much greater for the oxide of lead than that of the acetic acid, that it forms a subarseniate of lead in the liquid which contains acetic acid in excess. The subarseniate of iron not being soluble in it precipitates at the same time; for the strongest acids combine with the oxide of lead, and the acetic acid is at length the only free acid in the liquid. The solution which contains the oxides of nickel and lead, when mixed with sulphate of soda, suffers the greater part, but not the whole, of the oxide of lead added in excess to separate. If excess of ammonia be afterwards added, a greyish precipitate is formed composed of oxide of lead and oxide of nickel. In order to separate the last remains of the lead, sulphuretted hydrogen must be employed.

Hydrosulphuret of ammonia does not separate arsenic acid from oxide of nickel; for the precipitate produced is as soluble in an excess of hydrosulphuret as in caustic ammonia. The solution has a very deep yellowish-brown colour: when rather concentrated, it loses its transparency. Acids decompose it, but the precipitate contains some arsenic, and usually a small quantity of sulphuret of nickel is redissolved by the acid, even when it is only the acetic. If an ammoniacal solution of this sulphuret, which contains arsenic acid, be evaporated, a part of the sulphuret is deposited in the form of arseniuretted sulphuret of nickel (that is to say, a compound of sulphuret and of arseniuret of nickel), and another part oxidizes in proportion as the ammonia evaporates, and gives rise to a solution of nickel. The sulphuretted arseniuret of nickel is insoluble both in muriatic acid and in ammonia. Cobalt differs from nickel in being perfectly well precipitated by the hydrosulphurets, without being redissolved by an excess of them.

Oxide of Nickel and other Salifiable Bases.—It is well known that oxide of nickel dissolves completely in ammonia. This solution ought to be considered as forming a double subsalt; yet the hydrate of nickel is soluble in ammonia, although in a quantity much smaller than the oxide combines with an acid. The same affinity that determines the solubility of oxide of nickel in ammonia exists between it and other bases, although the combinations with these latter are insoluble in water. Consequently when a solution of nickel is mixed with another of an insoluble base, the ammonia occasions a precipitate, which contains nickel, and from which an excess of ammonia cannot separate this metal.

In this case the oxide of nickel is divided between the two bases, towards which it usually acts as an acid or electro-negative body. It is in this manner that it precipitates with the alkaline earths, the protoxide of iron, of manganese, lead, &c. But with the protoxide of iron and with alumina, it precipitates in the state of base or of electro-positive body, and on this account, these latter contain much less than the former. If the oxide of nickel contains small quantities of another base, it is very frequently difficult to discover what this base is. Barytes, strontian, and lime, are shown when a concentrated solution of the oxide is mixed with carbonate or sulphate of ammonia, which precipitate the earths, and form soluble salts with the oxide. Phosphate of ammonia cannot be employed to separate it from magnesia; for though phosphate of nickel is soluble in all proportions in ammonia, it precipitates nevertheless with phosphate of magnesia, without the possibility of separating it by ammonia added even in great excess. I know of no other method of separating them, than that of precipitating the oxide of nickel from the mixed solution by hydrosulphuret of ammonia, and of immediately decomposing the excess of the hydrosulphuret by a few drops of acetic acid, and filtering the solution. The magnesia remains in the solution, and may be separated from it in the usual manner; but in general, different methods must be followed with every base with which the oxide of nickel may be mixed.

It follows from what has been mentioned, that the solubility of oxide of nickel in ammonia cannot be employed to separate it perfectly from foreign admixtures insoluble in this alkali; for the parts undissolved or precipitated always contain more or less oxide of nickel.

The combination of oxide of nickel with caustic potash appears to me to be the most remarkable of all those which it forms with salifiable bases. When a solution of caustic potash is gradually added to one of nickel supersaturated with ammonia, a whitish precipitate appears, which redissolves, but which is eventually reproduced, and does not again disappear. When the potash ceases to render the liquor turbid, it becomes colourless, and a greenish sediment is slowly deposited. On this occasion, the oxide of nickel combines with the potash, and becomes insoluble. The redissolving of the first formed portions of the precipitate is derived from their decomposition by the ammoniacal salts; but at the moment in which the latter are decomposed, the precipitate remains undissolved. The precipitate is insoluble in ammonia. Separated by the filter, it forms a gelatinous mass, which it is extremely difficult to wash. If there is any lime in the liquid, the precipitate is less coherent, and more easy to wash; but it then contains all the lime remaining in the solution. Boiling water penetrates the *niccolate* of potash (*sit venia verbo*) much better; but it also decomposes it and separates the potash; and when the liquor which passes through the filter is evaporated to dryness, and leaves no residuum, the washed oxide is an hydrate of nickel which contains no trace of potash; if, on the contrary, there was an alkaline earth, it would remain combined with the oxide.

There is considerable difficulty in ascertaining whether the oxide thus obtained contains alkali or not. Thinking at first that the washed oxide still retained the potash precipitated with it, I endeavoured to analyze it in order to determine the quantity of potash retained; but all methods in the humid way completely failed. There remained then only to reduce the oxide by means of hydrogen gas, as I had done with the oxides of lead and of copper. One hundred parts of nickel, heated to redness before the experiment, gave me 78·8 parts of nickel, which, digested for a long time in water, did not impart to it the property of acting like an alkali; or, if there was any action at all, it was scarcely perceptible: then, as we know from other experiments that 100 parts of oxide of nickel contain 78·70 of nickel, it is evident that the oxide contained no potash. In another experiment I obtained from 100 parts of oxide of nickel, 79·7 of nickel; but this nickel imparted to water the property of acting as an alkali. This water became turbid by exposure to the air and on the addition of oxalic acid, it, therefore, contained lime. This oxide was obtained by an analysis, in which I had not separated from the ore the traces of carbonate of lime that it frequently contains.

Those bases which are soluble in ammonia possess the other properties of oxide of nickel to such an extent, that it is often difficult to discover them, especially when they are in small quantity: these bases are the oxides of cobalt, copper, and zinc.

Oxides of Nickel and Cobalt.—I have already observed that Mr. Phillips has given us a method of separating these two oxides by means of caustic potash. In order that it may succeed, it is requisite that the ammoniacal solution should be very dilute, and that the water with which it is diluted should be freed from atmospheric air by long ebullition; for the addition of potash gives the oxide of cobalt, which is dissolved, a strong disposition to become peroxide, which appears to depend upon the potash combining with the acids, the oxide of cobalt is held, therefore, in solution by the ammonia alone; whereas, before it was dissolved in the state of a double subsalt, consisting of ammonia and oxide of cobalt.

If the solution contains atmospheric air, the oxide of cobalt combines with its oxygen, and the oxide of nickel comes down with the peroxide formed. The more concentrated the solution is, the greater is the tendency of the oxide of cobalt to peroxidize; and it then frequently deposits during filtration. It is, therefore, easier to separate a large quantity of nickel from a small quantity of cobalt, than the converse, although in every case the quantity of oxide of cobalt thus carried down is too inconsiderable to alter in any notable degree the result of an analysis; especially as the two oxides possess the same saturating power and consequently the chemical constitution of the compound is easily discovered. If the oxide of nickel precipitated by the potash contains any cobalt, it becomes brown when any very dilute acid is poured upon it; for the peroxide of cobalt dissolves much more slowly than the oxide of nickel. It may be discovered also by means of the blowpipe, if the oxide of nickel be treated with borax until it is reduced, and the red colour which it gives to glass disappears. If it contains cobalt, it is then discovered by a more or less perceptible blue colour. I am of opinion that by Mr. Phillips's method, we may perfectly succeed in separating these two oxides, especially if the abovementioned precautions are observed.

The oxide of cobalt remaining in the ammoniacal liquor gives it a rose colour. By evaporating the solution, the oxide is deposited in brown flocculi, and may be collected by the filter. If the ore under examination contains silica in such a state that it may be dissolved, it now precipitates with the oxide of cobalt.

Oxides of Nickel and Copper.—I have not been able to determine whether oxide of copper is soluble in ammonia or not. It is certain that all those solutions which are generally regarded as oxide of copper in ammonia are double salts with excess of base. I digested oxide of copper in concentrated ammonia for eight days in a stopped bottle. The solution became of a light-blue colour in 48 hours, and it did not afterwards increase. A drop of carbonate of ammonia let fall into the liquor immediately dissolved a part of the oxide, and made the lower stratum of the liquid of a deep-blue colour.

When an ammoniacal solution of oxide of copper is mixed with caustic potash, the oxide of copper is precipitated in a few seconds, and if the quantity of the potash is sufficient, it is entirely deposited in the form of a blue hydrate, which it is very easy to wash. When well washed, it yielded blue hydrate of copper, combined with two atoms of water; it does not retain any trace of potash. In order that the copper may be perfectly precipitated from the ammoniacal solution, much more caustic potash must be employed than to separate oxide of nickel—a circumstance which is probably derived from *cuprate of potash* forming only in a liquid which is saturated to a certain degree with hydrate of potash; it is more easily decomposed than the *niccolate.*

I endeavoured to take advantage of these properties of oxide of copper to determine the quantity of copper in these analytical experiments. The method which has been always used to precipitate metallic copper by iron is extremely bad, and always gives incorrect results; for, on one hand, the copper is always mixed with carburet of iron, which separates from the precipitating iron in proportion as it dissolves; on the other hand, the reduced metal can scarcely be dried without suffering considerable oxidation. If the oxide of copper be precipitated by a carbonated alkali, this alkali, when added in excess, always dissolves a small quantity of carbonate of copper. This may indeed be obtained, if the solution be evaporated to dryness, and the residuum be made red-hot; the carbonate of copper is then decomposed, and water separates the alkaline subcarbonate from it; but then the oxide of copper covers the sides of the crucible, and adheres very firmly to it. The crucible must first be weighed alone, and then with the oxide of copper, which must be dissolved by an acid. No one of these methods is convenient. I have found that the method of separating oxide of copper from its ammoniacal solution by caustic potash gives a much more correct result than the foregoing processes. The separation is not, however, perfect; for the ammoniacal liquor, when filtered, becomes brown on the addition of hydrosulphuret of ammonia, and in a few days flocculi are deposited, but so inconsiderable in quantity that I could not weigh them with certainty. I also endeavoured to precipitate the copper from its solutions by means of sulphuretted hydrogen, and to weigh the dried bisulphuret; but it always gave me at least three or four per cent. too much weight for the oxide of copper employed, for the bisulphuret of copper becomes acid during exsiccation, as occurs with the similar sulphurets of rhodium and of platina. When distilled in a small apparatus to expel the excess of sulphur, the sulphuric acid, and moisture, the remaining protosulphuret of copper gives the quantity of copper with more exactitude.

But let us return to the mixture of oxide of copper with oxide of nickel. What I have already said of the analogy of these two oxides proves that the oxide of nickel, when precipitated by caustic potash from a solution which contains copper, must contain some of this metal, a part of which, however, still remains dissolved in the ammonia, unless a great excess of potash be added. It is, however, very easy to separate the copper from the nickel by sulphuretted hydrogen, which precipitates the former from its solution in an acid without acting upon the latter.

Oxides of Nickel and of Zinc.—The oxide of zinc dissolved by ammonia is also precipitated by an addition of caustic potash; but it precipitates more slowly than the oxide of nickel, and requires more potash. Its presence is discovered in oxide of nickel by reducing the latter by means of soda in the flame of the blowpipe. If it contains zinc, the charcoal is covered with a white incrustation of oxide of zinc; but to effect this, it is necessary to use a strong heat. In an analysis of a metallic mixture which I performed a long time since, I endeavoured to separate these two oxides by slightly heating their nitrates so as to peroxidize the nickel. I afterwards poured upon it dilute nitric acid, which dissolved the subnitrate of zinc, leaving the peroxide of nickel unacted upon. It is difficult to perform this experiment; for either too much or too little heat alters the results, and the separation is never complete, even when it succeeds well. Another process suggested itself afterwards. The mixture of the two oxides is put into a bulb blown in the middle of a glass tube, through which a current of dry muriatic acid gas is passed: the bulb is to be heated by means of a spirit lamp; the oxides combine with the muriatic acid; and the water, as well as the muriate of zinc distil, and may be received in a vial containing water. The muriate of nickel being much less volatile than that of zinc remains in the bulb. If the empty bulb be weighed before the commencement of the experiment, and after having put the oxide into it, it is necessary only to weigh it, to find the relative weights of the two oxides; but the muriate of zinc may also be precipitated by subcarbonate of soda, and its quantity determined in a direct mode.

(To be continued.)

ARTICLE XIII.

Astronomical Observations, 1822.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Jan. 14.	Emersion of Jupiter's first satellite.	6h	22′	27″	Mean Time at Bushey.
		6	23	48	Mean Time at Greenwich.
Jan. 29.	Emersion of Jupiter's second satellite	6	54	15	Mean Time at Bushey.
		6	35	36	Mean Time at Greenwich.
Feb. 6.	Emersion of Jupiter's first satellite.	6	40	52	Mean Time at Bushey.
		6	42	13	Mean Time at Greenwich.

ARTICLE XIV.

Meteorological Table. Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″. Above the Level of the Sea 129 feet.

1821.	Morning, 10 o'clock. *Mean height of*		Even., 10 o'clock. *Mean height of*		Mean temp. by Six's Ther.	Depth of Rain.	No. of days.	
	Barom.	Ther.	Barom.	Ther.		In. 100	Rain or Snow.	Fair.
Jan.	29·791	37·645	29·780	36·903	37·225	3·20	14	17
Feb.	30·131	40·750	30·124	38·928	40·357	0·60	7	21
March	29·465	42·096	29·425	39·774	41·290	3·50	18	13
April	29·510	49·366	29·503	45·200	47·366	3·35	16	14
May	29·768	50·193	29·758	44·935	47·838	1·70	15	16
June	30·779	56·666	30·112	50·866	54·800	0·50	6	24
July	29·784	59·161	29·786	54·709	58·419	1·10	12	19
Aug.	29·802	59·612	29·800	55·222	59·290	1·15	9	22
Sept.	29·642	57·366	29·630	54·066	56·666	2·10	16	14
Oct.	29·654	48·967	29·647	47·580	49·000	1·75	14	17
Nov.	29·463	43·233	29·487	41·100	42·633	5·25	20	10
Dec.	29·176	40·290	29·178	39·935	40·299	4·80	25	6
Aver. of year.	29·747	48·779	29·686	45·768	47·921	29·00	172	193

ANNUAL RESULTS.

MORNING.

BAROMETER.				THERMOMETER.		
Observations.		*Wind.*			*Wind.*	
Highest,	Jan. 23	W	30·74	Sept. 6	S	67°
Lowest,	Dec. 25	W	28·14	Jan. 3	S W	20°

EVENING.

Highest,	Jan. 22	N W	30·69	Sept. 3	S W	62°
Lowest,	Dec. 25	W	28·12	Jan. 4	N E	21°

Weather.	*Days.*	*Wind.*	*Times.*
Fair	193	N and N E	10
Rain or snow	172	E and S E	132
		S and S W	45
	365	W and N W	178
			365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, Jan. 3, Wind S W	12°
Hottest, Aug. 23, Wind S E	74°
Mean temperature for 1821	47·9315

Result of two Rain Gauges.	*In. 100*
Centre of Kinfauns Garden, about 20 feet above the level of the sea	21·18
Kinfauns Castle, 129 feet	29·00

Article XV.

Analyses of Books.

A Treatise on a Section of the Strata from Newcastle-upon Tyne to Cross Fell, in Cumberland, with Remarks on Mineral Veins in general, &c. &c. To which is added a Treatise on the Discovery, the Opening, and the Working of Lead Mines, with the Dressing and Smelting of Lead Ores. By Westgarth Forster. Second Edition. 1821.

No country in the world has carried the art of mining so far as England has done. Foreigners who have of late years visited our mining counties are struck with our efforts, the successful application of mechanism, and with the regularity of system that prevails in some districts; and yet when they inquire for an English book in which they may find these things explained, and see their history traced, they are surprised to learn that their inquiry is in vain, and that except some obsolete treatise, or detached paper in a journal, or in a Society's Transactions, nothing of the kind exists.

The title of the work now before us, which we have somewhat abbreviated, would lead us to expect that for a particular and for an important district, something had now been done; and though we may think that those whose experience is confined to one country, should not attempt an account of mineral veins in general, yet we readily admit that nothing would be more desirable than a record of the observations of practical men relative to those situations with which their knowledge is greatest. We wish, therefore, Mr. Forster had been guided more by such a rule, and we think that he might have avoided some errors, and have made a more useful book.

The part of the country under consideration is a curious one, and is singular for the number, extent, and regularity, of the beds, into which its stratification is divided, differing indeed most widely, though Mr. Forster does not seem willing to think so, from most other mining countries, which set at defiance the accurate sections which are such good guides to a Cumberland miner.

The upper series of these beds contain the coal of Newcastle, and in the lower series which *basset out* from under the coal measures, are found the valuable lead mining fields of Derwent and Allendale, in the county of Northumberland; Weardale, and Teasdale, in Durham; and Alston Moor, Nanthead, and Cross Fell, in Cumberland.

It is stated "that the general rise or acclivity of the strata, which is pretty well known to be in this part of our island to the south-west, and the dip or declivity to the NE. which in Cross Gill Burn, in Alston Moor, Cumberland, makes two degrees 15 minutes with the horizon, or nearly one yard in 27."

Thus by their rise to the westward, they crop out and present a succession to the observation of the naturalist, and to the research and labour of the miner, while "the stratification has," as is observed, p. 92, "been ascertained with the greatest precision by the multitude of shafts and workings of the lead mines," so that each individual bed is anticipated and calculated upon with the greatest confidence.

Thus indeed a valuable series of rules are established, which render mining here comparatively a simple process, and the miners of many parts of England would indeed have reason to rejoice, if they could be thus relieved of some of their greatest uncertainties.

The work consists of a preface—introduction—the treatise on the sections—on mineral veins in general—list of lead mines—on the opening and working of mines—dressing and smelting the ores.

We shall make some remarks on what occurs to us under each head as worthy of notice.

Of the preface, we have little to say, except with regard to one passage, wherein the author denounces theory as the bane of geological science, charging it "with having cramped the efforts of inquiry, and paralyzed the exertion of research."

Is not this rather too dogmatical and unphilosophical? and after all, is it true? We are not the advocates of any system, but has not an hypothesis often led to investigation? Has not a favourite, and, perhaps, an absurd theory often led to the collection, the arrangement, and the record of facts, and which but for this we should never have known? Can Mr. Forster take upon himself to say that the labours of De Saussure, Hutton, De Luc, Werner, Playfair, and Cuvier, would have been what they are without this stimulus, not to mention the tribe of other useful writers who have been called forth by their more splendid example?

The fact is, that the mind, as soon as facts are collected, tends to theorize, and the simplest deduction in geology is often but an hypothesis. Such language as the above is, however, rather the fashionable slang of a sect of geologists, and we are rather sorry to see a practical man fall into it.

The introduction had better have been omitted, or Mr. Forster might have submitted it to some of his mineralogical friends who would have given him a better classification of stones than that which disposes them into *scintallant*, or otherwise.

The treatise on the section is divided into two parts, and the first properly and naturally takes the upper beds into consideration, which are usually called the coal measures.

The general description of strata which it begins with, is clearly written, and is the more valuable, as it evidently results from the author's own knowledge, and includes, therefore, some of those minuter remarks for which we are always thankful to practical men.

We are amused to observe a note added, p. 16, relative to Mr. W. Smith, which is an old acquaintance, having appeared in almost the same words in various forms, and at divers periods, in the pages of the Phil. Mag. appended to Mr. Farry's contributions.

The explanations refer to the section, which is neatly executed, and which, under the head of coal measures, includes 90 beds, occupying a total thickness of 361 yards. In these are found 13 beds of coal, not reckoning such as are less than one foot thick, and making an aggregate depth of somewhat more than 13 yards of this substance.

The explanation itself is almost entirely composed of extracts from Mr. Winch's valuable papers in the Transactions of the Geological Society. This mode of filling out a book we strongly reprobate; in a new work the public have a right to new matter, and a reference to what has been published is sufficient.

Neither do we see the use of the number of tables of measurements of strata at different collieries, many of which are to be found in other publications, and are after all but of little general interest; nor do we like quoting from Williams's Mineral Kingdom a few tables of strata which are found to accompany coal in other parts of the kingdom, which relate merely to Whitehaven, Derbyshire, and one place in Scotland.

Thus in what should appear to be a comparative view of coal stratification, no notice is taken of the immense depositories in Staffordshire, Shropshire, or South Wales, nor is their most valuable accompaniment, ironstone, at all noticed, or its relation as to position and so on, or where it exists.

The second part of the treatise on the section, and which relates to the lead measures, begins with the following passage, which we extract as a favourable specimen of the author's composition:

"The strata which I shall now endeavour to describe is that part of the series which occurs in the lead district, comprising Derwent, East Allendale, and West Allendale, in the county of Northumberland; Weardale, and Teasdale, in the county of Durham; and Alston Moor, in the county of Cumberland. There are in this district two places where three counties meet in one point; viz. Rampgill Head, one mile south-west of Coal Cleugh and Caldron Snout, a waterfall on the river Tees. At the former of these places, the counties of Northumberland, Cumberland, and Durham, form a union; and at the latter, the counties of Durham, Yorkshire, and Westmoreland.

"This tract of country differs considerably in external appearance from that in which coal occurs so plentifully. The easy and natural undulations of the surface in the neighbourhood of Newcastle become exchanged for more rugged and alpine elevations; the fertile valleys of the Tees, the Wear, and the Tyne, are greatly contracted in breadth, and separated by sterile and desolate mountains, whose summits for a great part of the year are covered with snow.

"Among these mountains are distributed the various valuable lead mines, which constitute so large a part of the mineral treasures of Great Britain, and equal, if not excel in productiveness any yet discovered in the world."

Here we must stop our quotation to remind Mr. Forster that he is not writing a poem or a romance, but that, as the author of a book which may be hereafter referred to for facts, he ought to have been more careful. Before advancing so much, he should have inquired, and a very slight research would have informed him better.

That the lead mines in question can form so large a part of the mineral treasures of Great Britain as this sentence would imply cannot be true, if the iron, the copper, the tin, and the lead, of other districts be for a moment considered.

But as correctness of facts is of the first value in works of this kind, and to statistical inquirers is most important, we shall do our best to show how this matter stands.

Excluding the iron from our account, although both that and coal are mineral treasures of the very first importance, yet they are not derived from veins such as Mr. Forster had in his view; and, secondly, because we do not know any good estimate of the value of iron in this kingdom.

We shall confine ourselves then to the produce of the true mines of the metals, of which accounts may be procured.

We will first state the proportion of lead which these mines produce compared with that of the kingdom at large; and though from documents before us, we should have ranked them higher in this respect, yet we must of course take Mr. Forster's account to be correct. We wish that instead of a short average of the quantity of lead ore raised annually from 1800 to 1821, he had given us tables of each year's produce. Such tables would be very interesting, particularly when compared with prices preceding or succeeding changes of quantity.

We have, in the following statement, added two columns, one in which the ore is reckoned in pig lead, according to Mr. Forster's rule; and the second, in which the value is stated, taking it at £24 per ton, its probable value when smelted and delivered at the usual places of shipment; and we shall reckon the value of the metals from other districts in the same way.

Mr. Forster states the average annual produce ending with 1820, p. 420, as under:

	Bings of ore.		Tons of pig lead.	Value. £	s.	d.
Teesdale mines	8000	equal to	1778	42672	0	0
Weardale ditto.	17000		3777	90648	0	0
Allendale ditto.	8000		1778	42672	0	0
Alston Moor and Cross Fell	19000		4223	101352	0	0
Dufton Fell, Dun Fell, Silver Band and Hilton mines in Westmoreland	1500		333	7992	0	0
	53500		11889	285336	0	0

We will next state what we believe, from good authorities, and for some of which we can vouch, to be a near approximation to the quantities of lead produced in other mining districts in the kingdom.

	Tons of pig lead.	Value. £	s.	d.
Yorkshire	4900	107600	0	0
North Wales and Shropshire	6000	144000	0	0
Scotland	2000	48000	0	0
Derbyshire	5000	120000	0	0
Devon and Cornwall	1200	28800	0	0
	19100	448400	0	0

Thus it stands as under:

		£	s.	d.
Alston Moor, &c. &c	11889	285336	0	0
Other parts of the kingdom	19100	448400	0	0
	30989	733736	0	0

Here then we find that the mines in question produce about four-tenths of the *lead* of the kingdom, a large proportion certainly. We have still to estimate the extent of other mineral treasures, limiting ourselves as we have before mentioned. This can be done from more certain sources, and Mr. Forster would find accounts of the annual produce of copper and tin in Cornwall to a certain period in Dr. Price's book on the mines of that county, and he would see it also continued to the year 1810 in Rees's Cyclopedia. The quantity of copper made in England is likewise published every six months when the East India contracts are made, and may be seen in the Cornwall newspapers.

From sources of this kind, we are enabled to state that the produce of copper in the kingdom was in 1820 as under:

			£	s.	d.
Cornwall	6915	tons fine copper £112	774480	0	0
Devon, Anglesea, Staffordshire, &c.	1788	ditto	200256	0	0
	8703		974736	0	0

The tin of Cornwall and Devon was reduced in quantity about this period by a great depression in price, but it may be estimated at

3000 tons block and grain at £70.	210000	0	0

The total value of these metals of the kingdom may, therefore, be stated to be:

	£	*s.*	*d.*
Copper	974736	0	0
Lead	733736	0	0
Tin	210000	0	0
	1918472	0	0

To this ought to be added the value of silver, manganese, antimony, cobalt, zinc, but of which no probable estimate can be made.

So that the real proportion of the mineral treasures of the kingdom of this sort to be assigned to the district which Mr. Forster treats of, is about one-seventh of the whole.

Now as to its equalling or excelling in productiveness any part yet discovered in the world, we might mention the value of products of each of the three principal mining districts of Mexico, where we are told by Humboldt, that one mine only called Valenciana, yielded from the year 1771 to the time at which he was writing the annual amount of 600,000*l.*

But not to leave our own country, it will be seen in Mr. Thomas's short account of the mines, appended to his excellent map of the principal mining district in Cornwall, and which takes in only about 26 square miles, that the mines in that space produced in 1818, 55,920 tons of copper ore, which being reduced into copper at a medium rate of produce, and then valued, would amount to 516,656*l.* to which he mentions, in addition for tin, 41,880*l.* (valued in ore only, and this district not including the principal mines of this metal), and we have 558,536*l.* or just double the produce of all the Cumberland and Durham mines, and raised in a much smaller space.

In the compass of Mr. Thomas's survey are mentioned (table, p. 74) two mines, Dolcoath, and the United Mines, as producing the one 850, and the other 950 tons of copper ore per month; these together would make 1780 tons of fine copper in the year, worth near 200,000*l.* We do not know what the produce of that extraordinary spot in Anglesea, the Paris mine, at its best time, may have amounted to, but we conceive that if the copper then raised in a year was valued at present prices, which are not high ones, the money would be as much as that of a year's lead of Cumberland, &c.

In comparing individual mines with other lead mines, Mr. Forster unfortunately gives us but few data: he mentions, p. 274, Breconsike, as having formerly produced in some years 10,000 bings of ore, which would be 2250 tons of lead; and, p. 232, Hudgill Burn mine, is stated to be yielding 9000 bings, which would be 2000 tons of lead. We have reason to think that the produce of this rich mine has increased and is now near 3000 tons of lead.

But even this has been exceeded by other lead mines, one in Hulkin mountain in Flintshire, the property of Earl Grosvenor, produced within the last seven years 1900 tons of ore in a quarter; which would be at least 5000 tons of pig lead in the year. And in the same mountain, in the late Earl's time, there was at another mine at one period 3000 tons of ore dressed and washed ready for smelting.

We need not pursue this part of the subject further: we have noticed it particularly because we are of opinion, that writers on such subjects, who have it in their power, would render their books useful and interesting, if they would register facts which, by affording just comparisons, would lead to great additions to our statistical knowledge, the importance of which is so obvious: and further, because errors, such as we have noticed, are copied and distributed by other writers who quote them without being able to judge as to the real state of the case.

The great peculiarity of the country under consideration, is a stratification strikingly uniform in its arrangement and containing an extraordinary number of beds, which with unbroken continuity, prevail over a large extent of ground. Some of these beds are peculiar for the rich state of the lead veins that traverse them, while others above or below, though enclosing the same veins, have produced little or no metal.

A table exhibiting the relative productiveness of these beds, or a section in which by a figure the proportion of lead in each might have been shown, would have been a very interesting thing. We mean of course, that all that could be thus exhibited would be an approximation to the truth, which might, however, have been collected from the experience and opinions of the best miners. Mr. Forster tells us that the great limestone, which appears to be 21 yards thick, "(p. 103,) has been nearly as productive of lead ore, as all the other strata taken together."

The whole number of beds enumerated, which lie beneath the coal measures, reckoning from the deepest stratum of that series, down to the red sandstone, amounts to 148; and of these, 122 are classed under the head of lead measures: they consist principally of plate or shale, hazle or gritstone, and limestone, alternating with each other.

It appears to us from Mr. Forster's notice of the different strata, that lead ore is not found abundantly much under the tuft or water sill (a tender irregular gritstone) lying immediately under the great limestone, and the 38th bed of the series,—but this important fact, if it be so, is not explicitly stated.

As according to the statement that the great limestone has produced as much lead, as all the others taken together, and as this bed is 21 yards thick only, we may reckon, that very productive ground is limited to a space or thickness equivalent to the double of this, or about 42 yards, or in the language most usual in the English mines, to about 21 fathoms.

This will doubtless appear very extraordinary to other miners, who know no such limit to their operations, and who see thin veins rich in the metals, to depths that are only limited by the power that has as yet been applied to carry on the operations by which mining is pursued.

The actual thickness of the beds, which are productive of lead in various proportions, however, appears to be about 225 yards, or 112½ fathoms, as we infer, from a passage in p. 212, where it is said that they are those that are between the grindstone sill, and the five fathom limestone; and which, by referring to the section, we find occupy the above space But here is then, as above shown, one half the value contained in 10½ fathoms; and the other half, occupying 102 fathoms, would be of course better if it were also comprised in an equal space, and making together, as we have stated it, 21 fathoms.

This is a small allowance of workable ground, and it must be rich indeed to produce what it does, either in the number and extent of the veins, or in the degree in which these veins are replenished with ores.

Mr. Forster has, in the same page (212,) quoted Mr. Price, to show that the richest parts of the Cornwall strata are, for copper, from 40 to 80 fathoms deep; and for tin from 20 to 60; which would make it appear, that in other countries the metaliferous parts of the rocks were also limited to certain depths. Why has Mr. Forster referred to an authority written so long ago, and when probably such a notion did prevail, but which is certainly not true? There are many recent accounts of the mines in Cornwall, in papers in the Geological Transactions, by Dr. Berger, by Mr. Phillips, and others, and there are the sections before mentioned by Mr. Thomas, by which it will appear that the rocks in Cornwall are not divided into strata, in the sense the word is used in Mr. Forster's book, and that copper ore is found at all depths to which the powers of man have been able to follow it, and that no indication has presented itself by which it can be concluded, that it may not yet be pursued further, and by a continued application of the skill and energy which make the Cornish mines so remarkable—some of them being already full 200 fathoms under the level of the sea.

The account of the curiously stratified state of the district, exhibited in the section, is followed by some tables of strata in other places, as at Arkengarth dale, and Swale dale in Yorkshire; and also by a long extract from Mr. Farey's works, with respect to the rocks in Derbyshire. The inference which is intended to be drawn from all this seems to be, that the beds may be identified in all these different places. In our judgment, there is no ground whatever for such an opinion; but the gentlemen of the north are so pleased with the accurate way in which they can calculate on coming to a particular bed, that they are very apt to imagine that their rules would apply all over the kingdom, if people would but understand and use them.

Now in Arkengarth Dale are the chert beds, which are not to be found in Mr. Forster's section, and the only coincidence seems to be a bed of limestone of 12 fathoms thick.

Before we get to Derbyshire, there are mining fields not mentioned, such as Paitly Bridge, in Yorkshire, where, on Greenough Hill, it is limestone from the surface to the full depth of the mines, say 50 to 60 fathoms, and on the other side of the town, some extensive mines are in gritstone only. At Grassington Moor are a few beds of metalliferous gritstone alternating with plate, and under these is limestone already proved to a great thickness, and of unknown extent in that respect.

As to Derbyshire, it appears to us that in no place is stratification more irregular, nor do we pretend to understand Mr. Farey's arrangement of its parts, but it may be observed that on the western edge of the county is one of the deepest mines in England, we mean Ecton, and that is carried to the depth of 225 fathoms from the surface, and nothing occurs all the way down but limestone. The toadstone beds, we believe, ought to be there, but unfortunately they are not to be found. Mr. Farey calls this limestone, shale limestone, but that is only true in part.

The fact is, that no rules for mining in one country can be laid down as fit to be followed implicitly in another, and that a simple detail of things as they are, is what we think should be aimed at by authors of such works as the present.

We object also to swelling out a book by long extracts from other authors, and in particular we do not see why the account of the Huttonian and Wernerian theories should have been transplanted from Dr. Miller's edition of Williams's Mineral Kingdom.

Having now gone through the first and most important part of the book, the treatise on the sections, we must close our observations for the present. Z.

ARTICLE XVI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Jan. 31.—Observations on the Length of the Second's Pendulum at Madras, by John Goldingham, Esq. FRS.

Feb. 7.—Account of an Assemblage of Fossil Teeth and Bones belonging to extinct Species of Elephant, Rhinoceros, Hippopotamus, and Hyæna, and some other Animals discovered in a Cave at Kirkdale, near Kirby Moorside, Yorkshire, by the Rev. W. Buckland, FRS.

Feb. 14.—Mr. Buckland's paper was continued.

Feb. 21.—Mr. Buckland's paper was concluded.

This paper gives a detailed account of an antediluvian den of hyænas discovered last summer at Kirkdale, near Kirby Moorside, in Yorkshire, about 25 miles north-east of York.

The den is a natural fissure or cavern in oolitic limestone extending 300 feet into the body of the solid rock, and varying from two to five feet in height and breadth. Its mouth was closed with rubbish, and overgrown with grass and bushes, and was accidentally intersected by the working of a stone quarry. It is on the slope of a hill about 100 feet above the level of a small river, which, during great part of the year, is engulphed. The bottom of the cavern is nearly horizontal, and is entirely covered to the depth of about a foot, with a sediment of mud deposited by the diluvian waters. The surface of this mud was in some parts entirely covered with a crust of stalagmite; on the greater part of it, there was no stalagmite. At the bottom of this mud, the floor of the cave was covered from one end to the other with teeth and fragments of bone of the following animals: hyæna, elephant, rhinoceros, hippopotamus, horse, ox, two or three species of deer, bear, fox, water-rat, and birds.

The bones are for the most part broken, and gnawed to pieces, and the teeth lie loose among the fragments of the bones; a very few teeth remain still fixed in broken fragments of the jaws. The hyæna bones are broken to pieces as much as those of the other animals. No bone or tooth has been rolled, or in the least acted on by water, nor are there any pebbles mixed with them. The bones are not at all mineralized, and retain nearly the whole of their animal gelatin, and owe their high state of preservation to the mud in which they have been imbedded. The teeth of hyænas are most abundant; and of these, the greater part are worn down almost to the stumps, as if by the operation of gnawing bones. Some of the bones have marks of the teeth on them; and portions of the fœcal matter of the hyænas are found also in the den. These have been analyzed by Dr. Wollaston, and found to be composed of the same ingredients as the album græcum, or white fæces of dogs that are fed on bones, viz. carbonate of lime, phosphate of lime, and triple phosphate of ammonia and magnesia; and, on being shown to the keeper of the beasts at Exeter Change, were immediately recognized by him as the dung of the hyæna. The new and curious fact of the preservation of this substance is explained by its affinity to bone.

The animals found in the cave agree in species with those that occur in the diluvian gravel of England, and of great part of the northern hemisphere; four of them, the hyæna, elephant, rhinoceros, and hippopotamus, belong to species that are now extinct, and to genera that live exclusively in warm climates, and which are found associated together only in the southern portions of Africa near the Cape. It is certain from the evidence afforded by the interior of the den (which is of the same kind with that afforded by the ruins of Herculaneum and Pompeii) that all these animals lived and died in Yorkshire, in the period immediately preceding the deluge; and a similar conclusion may be drawn with respect to England generally, and to those other extensive regions of the northern hemisphere, where the diluvian gravel contains the remains of similar species of animals. The extinct fossil hyæna most nearly resembles that species which now inhabits the Cape, whose teeth are adapted beyond those of any other animal to the purpose of cracking bones, and whose habit it is to carry home parts of its prey to devour them in the caves of rocks which it inhabits. This analogy explains the accumulation of the bones in the den at Kirkdale. They were carried in for food by the hyænas; the smaller animals, perhaps, entire; the larger ones piecemeal; for by no other means could the bones of such large animals as the elephant and rhinoceros have arrived at the inmost recesses of so small a hole, unless rolled thither by water; in which case, the angles would have been worn off by attrition, but they are not.

Judging from the proportions of the remains now found in the den, the ordinary food of the hyænas seems to have been oxen, deer, and water-rats; the bones of the larger animals are more rare; and the fact of the bones of the hyænas being broken up equally with the rest, added to the known preference they have for putrid flesh and bones, renders it probable that they devoured the dead carcases of their own species. Some of the bones and teeth appear to have undergone various stages of decay by lying at the bottom of the den while it was inhabited, but little or none since the introduction of the diluvian sediment in which they have been imbedded. The circumstances of the case and its contents are altogether inconsistent with the hypothesis, of all the various animals of such dissimilar habits having entered it spontaneously, or having fallen in, or been drifted in by water, or with any other than that of their having been dragged in, either entire on piecemeal, by the beasts of prey whose den it was.

Five examples are adduced of bones of the same animals discovered in similar caverns in other parts of this country, viz. at Crawley Rocks near Swansea, in the Mendip Hills at Clifton, at Wirksworth in Derbyshire, and at Oreston near Plymouth. In some of these, there is evidence of the bones having been introduced by beasts of prey; but in that of Hutton Hill, in the Mendips, which contains rolled pebbles, it is probable they were washed in. In the case of open fissures, some may have fallen in.

A comparison is then instituted between these caverns in England, and those in Germany described by Rosenmuller, Esher and Leibnitz, as extending over a tract of 200 leagues, and containing analogous deposits of the bones of two extinct species of bear, and the same extinct species of hyæna that occurs at Kirkdale.

In the German caves, the bones are in nearly the same state of preservation as in the English, and are not in entire skeletons, but dispersed as in a charnel house. They are scattered all over the caves, sometimes loose, sometimes adhering together by stalagmite, and forming beds of many feet in thickness. They are of all parts of the body, and of animals of all ages; but are never rolled. With them is found a quantity of black earth derived from the decay of animal flesh; and also in the newly discovered caverns, we find descriptions of a bed of mud. The latter is probably the same diluvial sediment which we find at Kirkdale. The unbroken condition of the bones, and presence of black animal earth, are consistent with the habit of bears, as being rather addicted to vegetable than animal food, and in this case, not devouring the dead individuals of their own species. In the hyæna's cave, on the other hand, where both flesh and bones were devoured, we have no black earth; but instead of it we find in the album græcum, evidence of the fate that has attended the carcases and lost portions of the bones whose fragments still remain.

Three fourths of the total number of bones in the German caves belong to two extinct species of bear, and two-thirds of the remainder to the extinct hyæna of Kirkdale. There are also bones of an animal of the cat kind (resembling the jaguar or spotted panther of South America) and of the wolf, fox, and polecat, and rarely of elephant and rhinoceros.[*]

The bears and hyæna of all these caverns, as well as the elephant, rhinoceros, and hippopotamus, belong to the same extinct species that occur also fossil in the diluvian gravel, whence it follows that the period in which they inhabited these regions was that immediately preceding the formation of this gravel by that transient and universal inundation which has left traces of its ravages committed at no very distant period over the surface of the whole globe, and since which, no important or general physical changes appear to have affected it.

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[*] M. Rosenmuller shows that the bears not only lived and died, but were also born, in the same caverns in which their bones have been thus accumulated, and the same conclusion follows from the facts observed in the cave in Yorkshire.

Both in the case of the English and German caverns, the bones under consideration are never included in the solid rock; they occur in cavities of limestone rocks of various ages and formations, but have no further connexion with the rocks themselves, than that arising from the accident of their being lodged in cavities produced in them, by causes wholly unconnected with the animals, that appear for a certain time to have taken possession of them as their habitation.

GEOLOGICAL SOCIETY.

Nov. 2, 1821.—A letter from M. Brieslak on the Gypsum of Monte Seano was read.

The gypseous deposit of Monte Seano is covered by a bed of yellow arenaceous marle, of four or five feet in depth, in which are found many rounded masses of the same marle; some large irregular crystals assuming the rhomboidal form of gypsum, and a thin layer bed of whitish compact gypsum of a scaly foliated fracture. Under this marle bed, sulphate of lime appears in horizontal layers, varying in thickness from two or three inches to three or four feet; and interrupted in many places by thin strata of grey schistose marle, with veins of fibrous and granular gypsum. The sulphate of lime is penetrated with a bituminous matter, of a compact, granular, or foliated or fibrous texture, and for the most part of a grey colour, but sometimes approaching to black, which sometimes exhales by percussion or friction. The gypsum of this quarry is very remarkable for the great number of vegetable remains which it contains; but in general, the impressions of the leaves are so much broken, and the stalks so irregularly dispersed, as to render it difficult to determine the genera to which they belong. Prof. Moritti, however, discovered among them the leaves of the salix caprea, of the viscum album, and of the acer platanoides, plants which at present may be found growing in the neighbourhood of the quarry.

"Observations on the Species of Belemnites called Fusiform, on Fossils of the Cactus Tribe, and on the Opercula of the Fossil Echini," by Mr. Cumberland, were read.

From a close examination of numerous specimens of belemnites in the Stinchcome Quarry, near Dumley, in Gloucestershire, and especially of some large ones of the fusiform species, Mr. Cumberland was enabled to discover that these bodies were only the nucleus of the interior of the upper part connected by a cylinder with the alveolus that belonged to its smallest chamber; and in one specimen he observed a triform muscle which formed the apex of the pointed end of the cone of the belemnite, and of which he had before remarked some traces in the Oxfordshire specimens.

In the cut made for a new road to ascend Clifton Downs from the Hot Wells, Mr. C. met with a specimen of one of those stones which have been generally referred to the Cactus tribe; and in which a part of one of the supposed species or leaflets was *in situ*, but broken off about a quarter of an inch from the base of the depressed pustule, with which, however, it exactly fits.

Among the specimens of echinus in Mr. Cumberland's collection are two species, viz. the esculentus, and the cordiform, both of which exhibit opercula *in situ*, and another in which the interior with a cell to receive it when withdrawn, is manifest.

Dec. 7.—The reading of M. de la Beche's "Observations on the Geology of the Coast of France," was concluded.

From Fecamp to Cap d'Antifer, the cliffs are composed of chalk with flints, containing the usual fossils of the same chalk in England. From the latter point, similar strata resting on green sand extend to beyond the Chateau d'Orchet, when the sand disappears. That portion of the interior which is bounded by the coasts of the sea and Seine, consists of chalk covered generally by flint gravel.

The green sand forms the under part of the cliff as far as Cauville, where it appears to rest in marl containing green earth. At Cap de la Hêve, an inferior bed of iron sand, containing mica and silicious grains, and overlying blue marl, and marl stone, becomes visible. This green sand contains abundance of alcyonia, echinites, and other organic remains.

At Benerville, the green sand is wanting, but the Vaches Noires Cliffs between Villers sur Mer and Dives are capped by it; and it there rests partly upon coral rag, and partly upon oolite beds above a thick blue clay, corresponding in character with the Oxford clay, and contains a variety of organic remains, among which are some remarkable alcyonia that have been described and figured by M. Lamouroux. Inland, the prevailing rock, is a loose silicious sand, containing nodules of blue limestone, or chert, dispersed in layers. Near Lisieux, a thick stratum of whitish soft calcareous sandstone, containing green earth, is quarried under it for the purposes of building.

The blue marl and marlstone which rises under the iron sand at Cap de la Hêve is composed of marl and argillaceous limestone in alternate layers, and has so much the appearance of blue lias that it has been mistaken for it; but an examination of Hengueville Cliff shows it to rest on the oolite formation. At the latter place, it is about 150 feet thick. This stratum contains the fossil crocodiles mentioned as found at Havre, and described by M. Cuvier.

Some traces of Portland beds are observable above the coral rag at Hengueville cliff. The latter contains numerous fossil corals, echinites, &c. Below this, stratum appear in the under part of the hill between Tongues and Benerville, but the best section is afforded by the Vaches Noires cliffs, where the blue marl or clay, agreeing in its geological position with the Oxford clay, attains the thickness of about 300 feet. It exhibits various organic remains, among which are a fossil crocodile described by Cuvier, bones of the plesiosaurus, a fossil fish, septaria, ammonites, &c.

From Dives to St. Come, the coast is flat and sandy, with the exception of some low cliffs of forest marble between Lyon and Luc. From St. Come to St. Laurent, the calcareous sandstone with chert seams that accompanies the inferior oolite, is seen forming the top of the cliffs, rising gradually to the westward as far as St. Honorine, and thence ascending to the NNW. From Vierville to Grand Camp, the entire cliffs are composed of calcareous sandstone with chert; and the blue lias is consequently below the level of the sea.

The inferior oolite may be traced inland in a south-easterly direction from between Maisy and Isigny, in the neighbourhood of Bayeux, where it appears to rest upon quartz or gravel beds of the new red sandstone formation. From Bayeux to within three or four miles eastward of Villers, it rests upon lias; and from thence upon argillaceous slate and greywacke to between Thury-Harcourt and St. Laurent de Condel.

The first appearance of the lias eastward on the coast is between St. Come and Arromanche, under the calcareous sandstone with chert seams. From hence to St. Honorine, the lias occupies the lower part of the cliffs. At the latter place, it forms a curve, and dips NNW, disappearing to the W. of St. Laurent. In the interior, it may be traced in a south-easterly direction from Isigny to Villers, and beyond, a small portion of it is found resting upon argillaceous slate, until it becomes hidden under the inferior oolite. Between Isigny and Carenton, and between Carenton and the neighbourhood of Lestre, it constitutes the elevated ground behind the alluvial flat which separates the hills from the sea, and extends a considerable distance into the interior. At the last mentioned point, it rests upon the new red sandstone formation which appears on no other portion of the coast. The lias of this part of France precisely resembles that of the south of England, and contains similar organic remains.

In the department of Calvados, gravel beds, composed of rounded pebbles of quartz, constitute the most abundant strata of the new red sandstone formation, being associated with beds of silicious sand, of a whitish colour for the most part, and occasionally joined with red marl.

From Bayeux to Villers, the lias rests upon these gravel beds. In the department of La Manche, the new red sandstone occupies a considerable tract of country in the vicinity, and to the south-east of Carenton. At St. Jean, it rests upon argillaceous slate, and to the westward in the neighbourhood of St. Vaast, it is found in a similar position. At Litry, in the department of Calvados, it is supported by the coal measures.

The top of the Brugère de Crecy, on the road from Condé sur Noireau to Caen, is formed of a conglomerate consisting of rounded quartz nodules, from the size of a pea to three or four inches in diameter, agglutinated by a hard red argillo-silicious cement, and resting upon nearly vertical strata of argillaceous slate and greywacke, of which, part of the mountain is composed. Near St. Laurent de Condel the same porphyritic conglomerate rises through the oolite formation; and it is visible between Valagues and St. Vaast. It bears a striking resemblance to that associated with the new red sandstone formation in the neighbourhood of Exeter.

At Litry, ESE. from Bayeux, coal measures occur, resting upon argillaceous slate, and occupying an oval space about 1700 yards from E. to W. and 850 yards from N. to S. The direction is E. and W. and the dip 22° to N. In general, the quality of the beds is indifferent.

At May, between St. Laurent de Condel and Caen, the compact sandstone that is found overlying the transition limestone, and forming part of a large denudation in the oolite formation, has the appearance of old red sandstone. The beds vary much in thickness, are sometimes micaceous, and dip 45° to the N. Between Centaux and Langannerie a similar stratum appears resting upon quartz rock near the last mentioned spot.

The general character of the quartz rock that occurs in the departments of Calvados and La Manche is that of indurated sandstone passing in some instances into common quartz. It is found in beds varying from two to eight feet in thickness, and resting on each other; the colour passing from white or whitish grey to a red tint. The denudations in the oolite formation in this district are formed wholly or in part by quartz rock. Between Tourville and Mondrainville in the road from Villers to Caen, argillaceous slate and greywacke are observed resting on this rock, and dipping with it at about an angle of 45° or 50° to the north. At Falaise, the quartz rock is intersected by similar strata, and in the country between Valogues and Cherbourg, particularly in the mountain of Le Roule, which rises behind the latter town, the same appearances are observed. In the quartz rock of Le Roule, cylindrical bodies, like those mentioned by Dr. M'Culloch, as arising in the quartz rock of Glen Tilt, are occasionally found.

Of the south-western part of the department of Calvados, a considerable part is formed of argillaceous slate and greywacke, extending in a line which passes nearly W. from the neighbourhood of Perrien to Litry, in a south-eastern direction to Villers, and ESE. to Croix, whence it runs to the SE. and crosses the road from Pont d'Onilly to Falaise. The greywacke is not very abundant among the argillaceous slate, but may be observed in several places.

In the north-eastern part of the department of La Manche, the slate is in general of a similar character to that found in the former department.

At St. Vaast and Reville, the slate hills suddenly terminate upon granite, which resembles in its characters that of Dartmoor, like it containing large crystals of felspar, sometimes as much as two inches in length; and varying in colour from a grey to a light red tint, according to a change in the colour of the felspar. At Reville, the granite of the coast has a tendency to split in two directions, one E. and W.; the other N. and S. and to form large blocks, of which the angles are not right angles. The granite of St. Vaast and the opposite island is split into similar oblique blocks, and the fissures are in the same direction. At St. Honorine, a grey granite is found, of which that in the neighbourhood of Vine may be deemed a continuation.

On the coast of Calvados there are the remains of two submarine forests; one, namely, between Benerville and Villers sur Mer; and the other opposite St. Laurent, whose trunks and branches of trees cross each other in every direction, and the general appearance resembles very much that described by Mr. Horner as occurring on the coast of Somersetshire, near the river Parret, except that the trees are more fully decomposed, being used for manure by the country people of the neighbourhood.

From the preceding account it will be seen that the rocks of this part of the coast of France correspond in position, and very generally in structure and organic remains, with similar rocks on the coast of England, being probably the continuation of those which appear along the coasts of Dorset and Devonshire, and the Isle of Wight.

A paper was read on "A Freshwater Formation at Hordwell Cliff, Hampshire, and on the subjacent Beds from Hordwell Cliff to Muddiford. By Thomas Webster, Sec. G. S.

In this paper Mr. Webster stated, that having very recently examined this coast, he found that Hordwell cliff was not formed of the London clay as was generally supposed; and as he had stated from the accounts of others in a former paper published in the Transactions of the Geological Society; but that it was composed of beds analogous to the lower freshwater formation of the Isle of Wight. Under these beds, which dip to the E. is another of white sand, and below this in the next cliff to the W. appears the bed similar to the London clay, and which contains the well known fossils published by Brander. This forms also the inferior part of a part of the coast still further to the W. called the High Cliff, which reaches nearly to Muddiford.

This series of beds, being similar to those on the opposite side of the Island at Headen Hill, Isle of Wight, is considered by Mr. Webster as affording a strong confirmation of the opinion he had formerly advanced respecting the extent of the Isle of Wight basin.

Mr. Webster also enumerates several fossil freshwater shells which he found at Hordwell Cliff, and among other remains is a fossil capsule or seed vessel.

Jan. 18, 1822.—The reading of "A Description of Specimens collected on a Journey from Delhi to Bombay," by B. Fraser, Esq. was concluded.

The distance from Delhi to Bombay is about 720 English miles, but the author's deviations from the immediate route make his course amount to not less than 1000 miles. He apologizes for the incompleteness of his collection, and the accompanying memoir, by stating the difficulties which attended the conveyance of specimens, unfavourable, and other circumstances.

It is, the author states, generally known, that the central part of India, north of the Nurbuddah, and between that river and the valley of the Jumna and Ganges, rises gradually from north to south, abruptly from the west, and irregularly from the eastward, so as to form a sort of plateau, the southern portion of which, in the province of Matira, is elevated about 1600 or 1700 feet above the Nurbuddah, and about 2000 feet above the sea. The present memoir relates principally to the western and north western portion of this elevated tract.

The city of Delhi is placed upon a rocky ridge, about 120 feet in height, close to the river Jumna, and on the north-eastern verge of the plateau just described. The most northern point of the hilly region is at Tooham, south of Hansee, about 90 miles north of west from Delhi. This hill, which is about 700 feet in height, is composed of granite. The hilly country is terminated on the north-west by a long range of hills, which skirts immediately the great western plain, of which the sandy desert forms the principal portion.

The northern part of the tract described by the author is composed entirely of primary rocks, which are succeeded on the south by a very extensive trap formation stretching down the west of the Peninsula as far south as the neighbourhood of Goah, a distance of more than 500 miles. The extent of the trap formation to the eastward is not yet known; but the author supposes the primary rocks to be continued southwards, through the whole of the peninsula to Cape Cormorin.

At Delhi, the rock is quartz, and the same substance occupies a very large portion of the surface, to the south and west, constituting apparently the upper part of the mountainous tract, and frequently assuming the form of sharp insulated peaks, called by the natives "*dants*," or teeth, which are described as being in one place "of pure white, and glittering like snow."

Other primary rocks, granite, gneiss, mica slate, and clay slate, and in a few places granular limestone, are occasionally observed.

Dolomite, of a bluish-grey colour, is commonly used for building in the vicinity of Ambire and Taypore, and the white marble of Mokranna, about 35 miles north of Ajmere, is remarkable over all this part of India.

About 14 miles west of Ajmere, the primary tract is succeeded by a country comparatively plain; from within which, the primary range is seen extending to a considerable distance towards the north, and to the west of the south. This plain is diversified by sand hills, with clay in the hollows between them, and occasionally by barren high banks of hard clay mixed with "*kunken*," a term applied by the author to a peculiar sort of calcareous concretion, which he has not described in detail. The basis of the flat country seems to be sandstone of several varieties, but in general of a dull reddish hue; the beds sometimes rising into hills 300 or 400 feet in height. In several places all the buildings are formed of this reddish stone, and it colours all the water in the tanks. The sand appears to have been formed of the detritus of this rock.

Within the flat country, north and west of the primary mountains, many salt lakes occur, one of which, that of Sambur, north-west of Jaypore, supplies nearly the whole of Upper India with salt; the waters becoming impregnated during the rainy season to such a degree, that when the lake dries up, the salt is found crystallized in abundance under the mud which it deposits.

The hills about Joudpoor, the most western point to which the author's course extended, occupy a considerable space to the north, west, and south of that place, and are of very different appearance from those above described. They consist of claystone porphyry, which appears to repose on the sandstone.

In returning towards the south-east, "dentated peaks" of quartz were seen about Pahlee, and the country became more fertile; and in crossing the mountainous range already mentioned, about 70 miles south of the neighbourhood of Ajmere, the rocks were still found to be principally quartz, the peaks of which rose to about 2000 to 2500 feet above the plains to the west. The plateau in general in this place being about 700 to 1000 feet above the country immediately on the south.

About Odeypoor, the quartz lies upon reddish granite, which continues for some miles to the east, and is succeeded by a low range of quartz, extending to 50 or 60 miles from Odeypoor; after which no more primary substances were seen. Beds of compact limestone occur just below this quartz range, and occupy apparently a tract of considerable extent in the vicinity of Neymutch.

In this vicinity also, low hills, like artificial mounds, are observed; the commencement of the extensive basaltic district already mentioned, which, in its progress to the south, rises into numerous summits of remarkable structure and appearance. The upper part of the heights is generally perpendicular, with a rapid slope beneath; and the faces of the hills which, in some instances, rise to the height of 1500 feet, are divided by parallel and horizontal beds of basalt alternating with amygdaloid, which abounds in zeolite. In one place, about 15 or 16 such beds were distinctly observable.

A small hill near the bank of the Nurbuddah is crowned with basaltic columns, and less distinct appearances of the same kind were seen in other places. In one case, the basaltic rock was traversed by a dyke of very compact texture, resembling lydianstone.

The immediate bed of the Nurbuddah consists of basalt, but in the valley to the north of the river, a granitic compound, gneiss, and clay slate, were found *in situ;* the last in vertical strata ranging about NW. and SE.

The town of Baug, at a short distance from the river, is built on horizontal beds of sandstone, and the route, for six or eight miles, was over rocks of the same kind, of various shades of colour, red, yellow, and white, disposed in strata. In several of the hills, a bed of compact yellowish-grey limestone, containing caves, was observed above the sandstone, and immediately beneath the soil, resembling the limestone of Neymutch, already mentioned, about 140 miles to the north.

The trap range, south of the Nurbuddah, is of bolder features, but of the same materials and structure with that above described. Similar rocks were found along the route through the Candeish, a low tract surrounded on all sides by mountains; and the appearance and geological structure of the heights in all the part of the country agree precisely with those of the *ghauts* that bound the table land of the Peninsula to the westward, the singular forms of which have frequently attracted the observation of travellers.

Feb. 1.—The Annual General Meeting was held, when the following members were elected officers of the Society for the ensuing year:

President.—William Babington, MD. FRS.

Vice-Presidents.—Rev. William Buckland, FRS.; William Haseldine Pepys, Esq. FRS.; Henry Warburton, Esq. FRS.; and William Hyde Wollaston, MD. FRS.

Secretaries.—William Henry Fitton, MD. FRS.; and Mr. Thomas Webster.

Foreign Secretary.—Henry Heuland, Esq.

Council.—Hon. Henry Grey Bennet, MP. FRS.; Arthur Aikin, Esq. FLS.; John Bostock, MD. FRS. and FLS.; Henry James Brooke, Esq. FRS. and FLS.; Daniel Moore, Esq. FRAS. and FLS.; George Bellas Greenough, Esq. FRS. and FLS.; Major Thomas Colby, LLD. FRS. L. and E.; Augustus

Bozzi Granville, MD. FRS. and FLS.; Peter M. Roget, MD. FRS.; Thomas Smith, Esq. FRS. and FLS.; Charles Stokes, Esq. FRAS. and FLS.; and Philip Barker Webb, Esq.

Mr. Thomas Webster, Keeper of the Museum, and Draughtsman.

Article XVII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

We understand that the First Part of the Memoirs of the Astronomical Society of London will be ready for publication in a few weeks.

Practical Observations on Paralytic Affections, St. Vitus's Dance, &c. By W. V. Ward.

A System of Analytic Geometry. By the Rev. Dionysius Lardner, AM. of Dublin.

JUST PUBLISHED.

The Theory and Practice of Gas-Lighting, in which is exhibited an Historical Sketch of the Rise and Progress of the Science, and the Theories of Light, Combustion, and Formation of Coal; with Descriptions of the most approved Apparatus for generating, collecting, and distributing Coal Gas for illuminating Purposes. By T. S. Peckston, of the Chartered Gas-Light and Coke Company's Establishment, Peter-street, Westminster. 8vo. With 14 Engravings of Gas Apparatus. Price 1*l.* 1*s.* Boards.

Botanical Rambles, designed as an easy Introduction to the Science of Botany. 12mo. 4*s.*

A Journal of popular Medicine, explaining the Nature, Causes, and Prevention, of Diseases, the immediate Management of Accidents, and the Means of preserving Health. By Charles Thomas Haden, Surgeon to the Chelsea and Brompton Dispensary. 2 Vols. 8vo. 18*s.*

An Essay on the Symptoms and History of Diseases. By Marshall Hall, MD. 8vo. 6*s.*

The Principles of Medicine on the Plan of the Baconian Philosophy. Vol. I. on Febrile and Inflammatory Diseases. By R. D. Hamilton. 8vo. 9*s.*

A Description of the Island of St. Michael, comprising an Account of its Geological Structure; with Remarks on the other Azores, or Western Islands. By John Webster, MD. Royal 8vo. 13*s.*

Robison's System of Mechanical Philosophy. Edited by Dr. Brewster. 4 Vols. 8vo. With 50 Plates. 4*l.*

Works of the late John Playfair, Esq. with a Memoir of the Author. 4 Vols. 8vo. 2*l.* 12*s.* 6*d.*

A Geographical, Statistical, and Historical Description of Hindostan, and the adjacent Country, from the most authentic printed Documents deposited at the Board of Controul. By Walter Hamilton, Esq. 2 Vols. 4to. With Maps. 4*l.* 14*s.* 6*d.*

ARTICLE XVIII.

METEOROLOGICAL TABLE.

1822.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Hygr. at 9 a.m.	
1st Mon.									
Jan. 1	S E	29·88	29·74	44	32	—	11		
2	N W	29·90	29·74	41	32	—			
3	S W	29·90	29·28	38	33	—	24		
4	N E	29·96	29·28	39	33	—			
5	N	30·12	29·96	38	31	—			
6	N	30·12	30·05	38	28	—			
7	N W	30·21	30·05	37	30	—	—		☾
8	N	30·24	30·21	41	35	—			
9	N	30·25	30·24	45	30	—	02		
10	N W	30·31	30·24	45	34	—			
11	N W	30·39	30·31	47	39	—			
12	W	30·39	30·38	47	41	—			
13	W	30·38	30·34	48	39	—			
14	N W	30·34	30·21	47	38	50			
15	N W	30·30	30·24	41	29	—			○
16	N W	30·24	30·23	37	24	—			
17	S W	30·40	30·22	44	30	—	—		
18	W	30·43	30·40	43	33	—			
19	W	30·43	30·27	47	41	—			
20	W	30·39	30·27	49	36	—			
21	N W	30·50	30·39	48	39	—			
22	W	30·50	30·35	46	39	—			
23	S W	30·35	30·02	46	41	—	—		☽
24	S W	30·08	30·02	48	41	49	23		
25	N W	30·16	30·08	48	39	—	02		
26	N W	30·38	30·16	47	26	—			
27	N W	30·38	30·27	44	32	—			
28	W	30·27	30·27	51	32	—			
29	W	30·41	30·24	48	26	—			
30	N W	30·41	30·40	47	28	—			●
31	S W	30·40	30·28	54	36	30			
		30·50	29·28	54	24	1·29	0·62		

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

First Month.—1. Rainy morning: fine morning. 2. Fine: white frost in the morning. 3. Overcast. 4. Cloudy: fine night. 5. Fine. 6. Fine. 7. A little snow in the forenoon. 8, 9. Fine. 10. Foggy: calm. 11. Ditto. 12. Foggy morning. 13. Fine. 14. Very fine day. 15. Morning, fine and clear: day, fine. 16. Very fine day. 17. Snowy morning: cloudy day. 18. Morning, foggy: day, fine. 19. Cloudy. 20. Fine. 21. Very fine. 22. Foggy morning: fine day. 23, 24. Drizzly. 25. Cloudy. 26. Cloudy. 27, 28. Very fine. 29. Foggy morning: cloudy. 30. Fine. 31. Fine.

RESULTS.

Winds: N, 4; NE, 1; SE, 1; SW, 5; W, 8; NW, 12.

Barometer:	Mean height	
	For the month	30·202 inches.
	For the lunar period, ending the 15th	29·556
	For 12 days, ending the 9th (moon north)	29·812
	For 15 days, ending the 24th (moon south)	30·317
Thermometer:	Mean height	
	For the month	39·193°
	For the lunar period	39·938
	For 30 days, the sun in Capricorn	38·850
Evaporation		1·70 in.
Rain		0·62

Laboratory, Stratford, Second Month, 22, 1822. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

APRIL, 1822.

ARTICLE I.

Answer to the Review of the Sixth Edition of Dr. Thomson's System of Chemistry, in No. XXI. of the Journal of Science, Literature, and the Arts, edited by Mr. Brande. By the Author of that System.

THOUGH this Review appeared about a year ago, circumstances, which it is needless to state, prevented me from examining it till the month of February last. The accusations contained in it are of such a nature that I consider myself called upon to refute them; and I have only to regret that my answer was not drawn up immediately after the publication of the Review itself.

Common report has announced that the Review in question was written by Dr. Andrew Ure, of the Andersonian Institution, Glasgow; and several circumstances induce me to give credit to that report. With Dr. Ure, however, I beg leave to disclaim any concern. I should consider it as a superfluous task to attempt any vindication of myself from his aspersions. No publication of his can ever do me the smallest injury where the name of the author is known.

Mr. Brande being the acknowledged editor of the Journal of the Royal Institution, I am bound to consider the Review as containing his sentiments of me and my work. The fifth edition of my System of Chemistry was reviewed by him pretty much in the same manner as the present. I was urged at the time by several friends, for whose opinions I entertain the highest respect, to answer that attack. But after reading it over, the Review appeared to me to show so little knowledge of the subject, that I thought it better to overlook it altogether. Had the reviewer of my sixth edition confined himself to sneers against my skill as a chemist, or my competence to draw up a System of Chemistry, I should have considered it an useless undertaking to attempt the defence of a work, which has run through six editions, without any support whatever but its own merit; which has been translated into most European languages; which has, in some measure, stamped the character of every systematic treatise both in Britain and America, and even on the continent of Europe; and which has been sanctioned by the almost unqualified approbation of the most eminent chemists in Britain, France, and Germany.

But when Mr. Brande thinks proper to arraign my character as a man, and to accuse me of the basest and most profligate conduct; it is no longer in my power to remain silent. Silence indeed in such a case could scarcely fail to be construed into an acknowledgment of guilt. But as my real conduct has been the very reverse of what Mr. Brande has stated it to be; as I have uniformly prided myself in the honesty, sincerity, and independence of my character; as I have been at considerable pains to give credit to whom credit was due; as I have uniformly, both in my System, and in the *Annals of Philosophy*, while I continued its Editor, given the merit of every chemical fact to the original discoverer of it, as far as my knowledge of the subject enabled me to go; as I am not conscious of any wilful misrepresentation or twisting of facts to serve any particular purpose; I should consider myself as guilty of a kind of *felo de se*, if I were not to step forward in the present case in my own vindication. I owe it likewise to the University of Glasgow, to which I have the honour to belong; and to his Majesty, who bestowed on me the Professorship, which I fill, without any solicitation on my part, to show the world that neither my abilities, my knowledge, my industry, nor my character, render me unworthy of that situation, or of the kind and munificent manner in which it was bestowed on me.

It is some consolation at least to think that Mr. Brande has placed me in very good company. An attack upon Berzelius pretty much in the same style as the recent philippic against me, appeared in an early number of Brande's Journal. In another number we have a tirade against Gay-Lussac, scarcely less flippant, and about as well founded. The object seems to be to convince the public that all chemical knowledge is confined within the walls of the Royal Institution. I consider Mr. Brande's attack upon the College of Physicians, and upon the Universities of Oxford, Cambridge, and Glasgow, as connected with the same system. What renders these attacks more indecent and improper than they otherwise would be, is, that the Editor of the Journal has never had the benefit of a University education, and seems quite ignorant of the systems followed in these seminaries. For instance, when he says that chemistry is taught no where in Great Britain except in London and Edinburgh, he surely was not aware that the system of medical education is exactly the same in Glasgow as in Edinburgh; that the professors in Glasgow are at fully as much pains, and that the number of medical students is increasing annually at a rapid rate. He cannot have been aware that the three last chemical professors in Edinburgh (Drs. Cullen, Black, and Hope), had previously filled the chemical chair in Glasgow; and that almost all the additions which these gentlemen made to the science were made while they occupied a place in our University. The chemical course of lectures in Glasgow occupies six months, and the lectures are delivered on every day of the week, except Sunday. A separate hour is devoted to examine the class, and even practical experimenting is not neglected. Will Mr. Brande pretend that an equally extensive course is given at the Royal Institution?

The Review, which I am going to examine, is a most furious attack upon me from beginning to end, and denies me all credit whatever as an author, an experimenter, or a chemist. It is made up of different kinds of accusations, which are mixed together with some ingenuity and address; but which I shall make bold, notwithstanding the many witty sneers against my fondness for scholastic divisions, to consider and refute under three separate heads. 1. I am accused of being utterly incapable of writing English, and of being ignorant of the first principles of arrangement. 2. I have made many *false,* statements of facts, partly to injure the reputation of Sir H. Davy, and partly to promote my own absurd and erroneous chemical opinions. 3. My book is stuffed with innumerable errors into which I have fallen from being unacquainted with the elements of the science of chemistry.

These are heavy charges indeed. But what opinion will my readers form of the candour and gentlemanly feelings of Mr. Brande, if I show that the Reviewer, in order to give a colour to his accusations, has had recourse to direct falsehood, to pitiful prevarication, and to the stale trick of raising into proofs of ignorance what he must have been perfectly aware were errors of the press?

As my present object is not to make any attack upon Mr. Brande, but merely to vindicate myself, I shall pass over his introduction without animadversion; though such is the tendency to inaccuracy and misrepresentation that even there we find both in abundance when perfectly uncalled for. What can be more inaccurate than the statement that the French have been satisfied with a single elementary work on chemistry; or that no controversy, or want of politeness, has existed in chemical discussions for a number of years back? The pages of his own journal, not to go further, exhibit controversies of the most virulent kind; and gratuitous attacks upon the character and abilities of some of the first chemists of the age. But let us examine the accusations contained in the review with some minuteness.

I. *Arrangement and Style.*

I shall quote the passages of the Review which allude to my arrangement and style, and subjoin to each of them my remarks.

"We are at a loss to learn why a new edition has come forth. It was not spontaneously called for, and nothing but a decidedly superior work should have been tendered to the public."—(Review, p. 126.)

To this I answer that the book is not my property, and that the new edition was published not by me, but by the bookseller to whom it belongs. I was obliged by my agreement to correct the press, and nothing more; and in the present case, as the book was printed in London while I resided in Glasgow, even this task was but imperfectly accomplished. The new edition, I presume, was printed because the old had been sold. I am not aware that booksellers proceed in any other way. Indeed I had reason to know that the book was out of print, because several of my own students had informed me that they could not procure a copy. Why it was necessary that the new edition should be decidedly superior to the old, I do not perceive. Nor could any blame have been attached to me, though it had been printed verbatim from the fifth edition. But it is in reality decidedly superior; because it contains all the additions which had been made to the science in the interval between the two editions, as far as they were known to me; and that my statements were pretty complete is evident from this, that neither Mr. Brande nor Dr. Ure, though they have both published books since my sixth edition appeared, have introduced a single topic, so far as I can observe, which I had not previously handled. What is the discovery which I have omitted to notice? The assertion of the Reviewers that it is ten years behind the present state of the science is remarkable only for its shameful falsehood. Not a single proof is advanced in support of it, except that I took no notice of the newly discovered connexion between electricity and magnetism. Now I was the first individual in Great Britain who made known Prof. Oersted's discovery. It appeared in the *Annals of Philosophy* for October, 1820. Before I became acquainted with this discovery, the whole of the sixth edition of my System was printed. Indeed, as the book was published in October, or soon after, Mr. Brande must have been aware of the absurdity of the accusation; and he must have been induced to bring it forward because he had no real omission to point out.

But had I even omitted some of the minor discoveries, it would not have been very surprising; nor, situated as I was, would the omission have been very culpable. I may, therefore, take some credit to myself for the great pains which I took to insert every novelty deserving of notice. To expect that I should write anew the whole book was very unreasonable. It would have been a task which I had no motive whatever for performing. I undertook merely to insert every thing which I considered as worthy of notice in its proper place. The repetitions to which the Reviewer alludes are exceedingly trifling, and do not altogether amount to 10 pages. The assertion that the second volume is a repetition of the first is so palpably untrue, that the Reviewer must have been aware of its inaccuracy when he made it. The first volume consists of 580 pages; the second of 722. Of these, there are 556 pages which treat of substances not so much as mentioned, or even alluded to, in the first volume. Now were the remaining 166 pages printed verbatim from the first volume, it would not be true that the second volume, or even the greater part of it, is a repetition of the first. But the fact is, that not a single page of repetition is to be found in the book.

What the Reviewer has thought proper to call repetition is, I conceive, attended with considerable advantage to the reader. In the first volume I give in a few lines the essential characters of the different mineral acids and bases; while, in the second volume, these bodies are treated of in detail, and their properties and history given at length. I find great advantages resulting from this plan of teaching the science; and should be happy to have an opportunity of comparing the progress of some of my own students with those of Mr. Brande.

"We proceed to the second division of the first book of his system, comprehending ponderable bodies, which are handled in a very *heavy* style."—(Review, p. 140.)

"By the aid of many italics, the Doctor tries in vain to give emphasis to his favourite mode of writing, which, from its extreme rarefaction of ideas, might be called the *vacuous*."—(Ibid. p. 129.)

"The whole information contained in his four papers on the specific gravities of the gases and the true weights of the atoms might have been easily conveyed in one-twentieth of the compass."—(Ibid. p. 125.)

The want of discernment evinced in these attacks upon my style occasioned some surprise at first. I may be very often accused of great carelessness of style; but never, unless I deceive myself egregiously, either of want of energy or diffuseness. Indeed the characteristic properties of my style are just the opposite of diffuseness. I am remarkably concise, though I hope always clear, and generally energetic. Nothing indeed can constitute a greater contrast than my mode of writing, and that of Mr. Brande. If he be a good writer on scientific subjects, it follows as a necessary consequence that I am a bad one. I refer the reader to his History of Chemistry in the Supplement to the Encyclopædia Britannica, to his article Chemistry in the same book, and to some of his prefaces in the Royal Institution Journal. In point of diffuseness, want of energy, and bad taste, these dissertations constitute a perfect contrast to every thing which ever flowed from my pen. Indeed were I disposed to criticise style, nothing would be easier than to retaliate upon Mr. Brande.

As to the nature of my own style I am very willing to let it rest upon the four papers which the Reviewer has stigmatized in the passage just quoted. These papers, with which the Reviewer in fact had nothing to do, occupy 67 pages of the *Annals of Philosophy*. They contain the details of my experiments to determine the specific gravity of 21 gases, and the atomic weights of 13 important bodies. I had to discuss the experiments of preceding writers, and to endeavour to point out the causes of the difference between their results and mine. Thus every individual substance occupies, at an average, almost exactly two pages. All these experiments were made with a degree of care and attention, which, I confidently affirm, has never been surpassed. In opposition to the Reviewer's statement, that "there is scarcely a single determination of Dr. Thomson's on any chemical subject of difficulty, during the last eight years, which has not been reversed," (p. 122.) I venture to assert that the determinations in these four papers, and in several more since published, will withstand all the attacks of Mr. Brande, however violent and long continued they may be; that they belong to one of the most difficult and most important parts of chemistry; that they are essential to the very foundation of the science; and that they have established the atomic theory upon a foundation which cannot be shaken.

"Over all the British compilers, Dr. Thomson claims precedence. Some of the others are content to transcribe from his collection, but he seldom or never condescends to pay any of his brother compilers a similar compliment. Possessing the minute patience of an index framer rather than the enlarged capacity of a systematist, he has contrived to bring together a great number of chemical facts, with copious references, convenient to the student, and imposing on the general reader; but in our opinion not entitling his work to be called a System of Chemistry."—(Review, p. 121.)

The inference, I presume, which the Reviewer wishes to be drawn is, that Mr. Brande's little elementary work, or his article Chemistry in the Supplement to the Encyclopædia Britannica, is entitled to be called a System of Chemistry; and that the same name may be bestowed upon the new edition of Nicholson's Dictionary.

The attack upon my arrangement awakened my curiosity, and induced me to turn to Mr. Brande's article Chemistry in the Supplement to the Encyclopædia Britannica. As I expected something very superior, the reader may judge of my surprise when I found he had adopted the following arrangement:

Part I.—Attraction, Heat, Electricity.
  II.—Radiant Matter.
  III.—Undecompounded Bodies.
    1. Acidifying Supporters of Combustion.
    2. Acidifiable Combustibles.
    3. Metals.
  IV.—Vegetable Chemistry.
  V.—Animal Chemistry.

Would not the author of such an arrangement have acted more wisely, if he had not directed the attention of the public to arrangement at all? Do not parts first and second clash with each other? Does not the first part include the second? The division of the simple bodies is obviously my old one. The very terms have been borrowed from me. I abandoned it, because the recent discoveries, for which we are chiefly indebted to Sir H. Davy, have broken down the line of distinction between the simple acidifiable combustibles and the metals: for example, arsenic burns as readily, and at as low a temperature, as charcoal itself. It is acidifiable too; for arsenic acid is a powerful acid, and neutralizes the bases as completely as any acid whatever. What can be more preposterous than to class arsenic and tellurium, bodies which enter into gaseous compounds with hydrogen (precisely as phosphorus, sulphur, and selenium, do), with a set of bodies which enter into no gaseous compounds whatever? He who does not perceive that arsenic and tellurium have a much closer resemblance to sulphur and selenium than to any metal, may indeed amuse the ladies and gentlemen who attend the Royal Institution; but is not likely to make any arrangement calculated for the improvement of the science.

My arrangement of the simple bodies was not made without the most careful reflection. If I erred, I must acknowledge that my error was not the effect of haste. Plausible objections may indeed be made to several parts of this arrangement. These objections I examined with all the care of which I was capable, before the fifth edition of my System was put to the press. I am still of opinion, that it is upon the whole the best of which the science in its present state is susceptible. I have divided the simple bodies, as the reader will find by consulting my System, into three sets, viz.

1. Supporters.
2. Incombustibles.
3. Combustibles.

The supporters are three, *oxygen*, *chlorine*, and *iodine*. To these I have added a fourth, *fluorine*, which is still only conjectural. No doubt chlorine and iodine might be placed among the incombustibles, as has been done by the French chemists; but I think that, upon the whole, these bodies bear a much closer resemblance to *oxygen* than to sulphur. At any rate Mr. Brande has no right to find fault with this part of my arrangement since he has followed it himself.

The only simple incombustible known is *azote*. The Reviewer is very facetious at my stupidity in believing azote to be incombustible. And I shall willingly come over to his opinion as soon as he shows me that he can set it on fire.

What pitiful quibbling is he guilty of in order to make out the combustibility of this substance! Were it really combustible, no fire could be kindled without exploding and destroying the whole air of the atmosphere. The very circumstance that atmospheric air has continued unaltered, notwithstanding the infinite number of fires which have been burning for these six thousand years, I hold to be a decisive proof that azote is incombustible. I have given an explanation of what I mean by *combustion*, in my System, and the meaning which I affix to it coincides with the usual acceptation of the word. If the Reviewer chooses to take the word in a different sense, what have I to do with that? He might with just as much propriety substitute the word *heavy* for *light*, and then affirm that hydrogen gas is the *heaviest* body in nature, and ridicule me for describing it as *light*.

The nature of the acids formed by the union of azote and oxygen is quite different from that of those formed when oxygen and combustible bodies unite. They approach more nearly to the acids formed by the union of oxygen with chlorine and iodine. Indeed azote may be considered as approaching as near the supporters of combustion as it does the combustibles. But it coincides with neither, and must, therefore, in the present state of our knowledge, stand by itself.

The simple combustibles I have divided into three sets; namely,

1. Acidifiable Combustibles.
2. Alkalifiable Combustibles.
3. Intermediate Combustibles.

The bodies constituting the first set are converted into acids when they combine with some supporter of combustion. They are all capable of entering into the composition of some gas or other; while none of the other two sets enter into any gaseous compound at all.

I have been blamed by some for placing arsenic and tellurium among the acidifiable combustibles; but my reasons appear valid. As for osmium, I was uncertain where to place it, and consulted Dr. Wollaston, whether he thought it likely that it belonged to the first or second set. It was his advice that led me to place it where it stands. The position is only provisional, till a set of experiments be made to determine its true situation, of which at present we know nothing.

In his Review of my fifth edition, Mr. Brande ridiculed me for considering silica as an acid. As this ridicule has not been repeated in the new review, we may presume that he has seen the propriety of the arrangement. Indeed this improvement was not made by me; but by Mr. Smithson and Professor Berzelius. I was satisfied of its justice by their arguments; and the number of silicates which I have described in my sixth edition cannot, I think, leave any doubt on the subject in the mind of any well informed chemist.

I have been blamed for not classing chromium, molybdenum, tungsten, and columbium, with the acidifiable combustibles. My reason for leaving them out was that they do not enter into any gaseous compounds, and that they decidedly belong to the class of intermediate combustibles.

The *intermediate combustibles* are those bodies which have the property of forming both *bases* and *acids* when they unite with oxygen. In one proportion, they form compounds capable of uniting with acids, and, therefore, perform the function of alkalies. In another proportion, they form compounds capable of uniting with alkalies, and, therefore, perform the function of acids. Hence they cannot with propriety be classed either with the acidifiable or alkalifiable combustibles, but are intermediate between both. It is not unlikely that some bodies may deserve a place in this class which I have ranked with the alkalifiable combustibles. Thus manganese seems capable of forming an alkali when united to a minimum of oxygen, and an acid when united to a maximum. My reason for leaving it in its old place was a wish to verify the recent experiments respecting manganesic acid; and this I have not yet found leisure to do.

These remarks will supersede the necessity of noticing particularly the vast mass of abuse which the Reviewer has contrived to heap together. The first volume of course contains the whole of my arrangement, because it contains the whole of the simple bodies. The science is not yet far enough advanced to admit the compound bodies to be systematically arranged. I have adopted the plan which appears to me most convenient for the student; but other methods might be adopted, and in fact have been adopted by others.

The sneers in which the Reviewer so frequently indulges respecting my general observations are most uncandid. Every one of the observations to which he alludes was rigidly correct when I first wrote it. Subsequent discoveries have, in some cases, introduced one or two exceptions. These I have not always had it in my power to notice. I was anxious to swell the book as little as possible, and did not scruple to pass over the few existing exceptions; because an attentive reader of the first volume was enabled without difficulty to state them for himself I am not willing to abandon the character for precision, which I have long enjoyed, though the Reviewer has thought proper to call it in question: on the contrary, I flatter myself that I possess it in no common degree. If some of my earlier experiments were deficient in correctness, the reason was, that minute accuracy in chemical analyses did not at that time appear to me an object of primary importance. The knowledge of the atomic theory has altered my views in this respect. All my recent experiments have been made with the most scrupulous attention, and the results which I have given in seven different papers on the specific gravity of gases, and the atomic weights of bodies, are as near the truth, as it was possible for me to go with the apparatus which I employed.

Let not this statement be warped (as the Reviewer has done) into an insinuation that I lay claim to any superiority in experimental dexterity. So far from this, I consider the accuracy of my results to be in reality owing to my want of dexterity; for it obliged me to look out for a method in which no dexterity was required. There is little difficulty in procuring the substances to be experimented on, pure. There is little difficulty in weighing the quantity wanted, true to the hundredth part of a grain; in dissolving it in distilled water; and in mixing two such solutions together. Such is the whole process. Any person, however little dexterity he may possess, will succeed in such experiments, if he be at the requisite pains. For example, I weigh 11 grains of sulphate of potash, and 13·25 grains of chloride of barium; dissolve each respectively in distilled water, and mix the solutions. After the sulphate of barytes has subsided, I test the clear supernatant liquid by mixing a little of it first with muriate of barytes, and next with sulphate of soda. Not the least opalescence is produced by either. Hence I conclude that the liquid contains no sensible quantity either of sulphuric acid or of barytes. This experiment, simple as it is, determines the composition of sulphate of barytes to be:

Sulphuric acid	5·0
Barytes	9·75
	14·75

And demonstrates that 5 and 9·75 respectively represent the atomic weight of sulphuric acid and barytes.

The Reviewer asks with a sneer (Review, p. 124), whether the preceding experiments of Berzelius, Wollaston, &c. are to be considered as good for nothing, and whether they are to be superseded by mine. I beg leave to ask in my turn whether the experiments of Bergman, Wenzel, Kirwan, and Richter, were good for nothing, and whether they are to be superseded by those of Berzelius, Wollaston, &c.? The object of every experimenter is to discover the truth; but from the imperfect nature of his apparatus, he only makes an approximation. His result serves his successors as a point from which they are enabled to set out; and if they be at the requisite pains, the labours of their predecessor will almost always enable them to approach somewhat nearer the truth than he did. To the labours of Berzelius I have always acknowledged myself greatly indebted. They are (all things considered) surprisingly accurate; nor should I have been able in many cases to have obtained good results without their assistance. If I have come nearer the truth than he has done, it was only because I was enabled to profit by his experiments.

As for Dr. Wollaston, the introduction of his name is most uncandid. His scale of chemical equivalents was constructed not from his own experiments, but from those of others. He examined them with his usual sagacity, and the numbers which he pitched upon approach in general very near the truth. His paper contains only a single experimental result of his own; namely, the composition of saltpetre; by which he determined the equivalent for *nitric acid* and *azote.* Now the fact is, that I have adopted almost the very atomic weights of these bodies which he had previously given. My obligation to him for these numbers had been distinctly stated in my former papers; consequently I had no occasion to allude to the subject again. Besides, in my paper on the specific gravity of the gases, I deduced the atomic weights of azote and its compounds in a different way; but in a way which I consider as very satisfactory.

As a specimen of the uncandid way in which my observations have been represented by the Reviewer, I may notice the ridicule which he throws on my statement, that acids are compounds of a supporter and combustible or incombustible. Now I ask, is not this observation true with a very few exceptions? We are at present acquainted with 50 acids to which it applies correctly. There are two acids, the *chloric* and *iodic,* which are composed each of two supporters of combustion. In these two acids, I consider the *chlorine* and *iodine* to act precisely the part which azote does in nitric acid. This is the view which the French chemists have taken, and it has induced them to place both chlorine and iodine among the combustibles. I have given my reasons for preferring my own arrangement; but I admit that in these acids the chlorine and iodine act the part of simple incombustibles. I do not, therefore, regard their existence as an exception to the general law, but as proving that chlorine and iodine may be considered under two points of view, either as supporters of combustion, or incombustibles. It is this double capacity of these bodies that constitutes the great distinction between them and oxygen.

There is another set of gaseous bodies capable of uniting with bases, and often on that account considered as acids. These are the compounds of hydrogen with sulphur, selenium, and tellurium. Now these bodies I have expressly separated in my System. Whether my reasons for this separation be conclusive, I shall not at present inquire; but it must be obvious to every one that if these three bodies be excluded, my general observations are precise.

I think it unnecessary to notice or refute the numerous and sweeping attacks upon several of my chapters; because they sufficiently refute themselves. For example, my account of combustion is said to be absolute *verbiage.* I have only to say, that I have given the best account of it which I could; and upon looking into the writings of Mr. Brande upon the same subject, I cannot find that they contain more, or indeed nearly so much information, as mine. I have given an historical view of the different opinions respecting combustion, which have been successively adopted by chemists. And whatever Mr. Brande may think on the subject, I must be allowed to retain my opinion that these historical details constitute some of the most instructive, as well as entertaining articles; and that they are very well calculated to rectify our own views. Had this gentleman made himself better acquainted with the history of the science, he would have avoided several awkward mistakes into which he has fallen.

The Reviewer panegyrizes Sir H. Davy's researches on flame. I agree with him in opinion that the experiments contained in that paper, like all the other experimental researches of that gentleman, are extremely valuable. They are characterized by that mixture of invention and dexterity which so eminently distinguish all his productions, and which have deservedly raised him to so high a rank among modern chemists. But it would be rather singular if these experiments should be considered as a reason for passing by in silence all the laborious investigations of so many chemists, who have preceded Davy, and who have accumulated a much larger collection of facts than it was possible for him to do. I am not aware of any new general principle deduced by Davy from his experiments, which ought to alter our previous opinions respecting combustion; and for my own part I must confess that after all that has been written on the subject by Berzelius, Davy, and even Mr. Brande, the theory of combustion is still a desideratum. I have advanced a conjecture on the subject, which the present state of our knowledge enables us neither to confirm nor refute. The Reviewer may ridicule this conjecture if he pleases; but this is a task which he will find much easier than to refute it.

II. *False Statements of Facts.*

I come now to what I consider as by far the most important part of the Review; because it is a direct attack upon my character. The Reviewer has the effrontery to affirm, that I have mis-stated various facts on purpose, in order to gratify certain malignant passions of my own, and to injure certain individuals of whose reputation I was meanly jealous. After Mr. Brande's attacks upon Berzelius and Gay-Lussac, which, if they meant any thing, went to accuse them of similar conduct, I had no reason to be surprised at his advancing such an accusation against me. But this does not preclude the necessity on my part of vindicating my character.

"Dr. Thomson's attacks on the exalted reputation of the President of the Royal Society have long excited our surprise and indignation, and as we observe them still persevered in, and still unanswered, we shall use our humble endeavours to expose their injustice and futility."—(Review, p. 122.)

This impudent assertion the assertor knew to be false when he made it, and has betrayed his knowledge in the very review in which it is contained. It is false that I have ever made any attack either on the character or reputation of Sir H. Davy. On the contrary, I have always been in the habit of reckoning him among the number of my friends. I have always spoken of his talents and of his labours with that respect which I felt for them, and have always been proud to think that his discoveries have reflected a lustre upon the country in which they originated. As an Editor of a journal, and as a chemical writer, I have laid it down as a rule, to be impartial; and never to allow my private feelings, whatever they were, to influence my judgment. This conduct, in which I mean to persist, and in which I shall always glory, has drawn upon me, it seems, the formidable resentment of Mr. Brande, who has magnanimously volunteered to expose it to the obloquy of the scientific world. The accusations are seven in number. I shall examine them one by one. The reader will observe that these accusations, which fill a considerable portion of the Review, have nothing to do with the merits of my System of Chemistry. They have been pulled in head and shoulders by the Reviewer as topics on which he thought that he could descant with some pathos and effect. I feel obliged to him for his accusations. Satisfied that my conduct as an author and an editor will bear the strictest scrutiny, and that these accusations will only tend to raise my character instead of injuring it, I shall proceed to examine the validity of each.

1. I have stated that "Sir H. Davy has embraced the Daltonian theory with some modifications and alterations of terms; but his notions are not quite so perspicuous as those of Mr. Dalton, and they do not appear to me so agreeable to the principles of sound philosophy."—(*Annals of Philosophy*, ii. 33.)

I cannot for my part conceive any thing more innocent than this passage, and am unable to discover the attack upon Davy, which it seems lies concealed in it. I was warranted in saying that Davy had embraced the principles of Dalton, because I knew it to be the fact. Both Dr. Wollaston and Sir H. Davy will recollect a long conversation which we had on the subject, after dining at the Royal Society Club in the summer of 1807, Both Dr. Wollaston and myself attempted in vain to convince Davy that the doctrine of definite proportions was not a chimera. What led Davy afterwards to embrace the doctrine, I need not here state, though I am acquainted with the whole history. If Mr. Brande has any curiosity on the subject, either Dr. Wollaston or Mr. Davies Gilbert can inform him of the particulars.

My statement then is the mere annunciation of a matter of fact without any intention whatever of hurting the feelings of any one. I knew from Davy himself that he had no idea whatever of definite proportions till Mr. Dalton had made known the outlines of his theory. Even five years after that period, Davy had not embraced it; and I was aware of the influence which had at last induced him to adopt it. With the knowledge of all these facts should I have acted honestly, if I had not stated that the atomic theory originated with Dalton. Surely Sir H. Davy's reputation, and his character as a chemist, stand sufficiently high to render it unnecessary for him to seek to bolster up his reputation by laying claim to the discoveries of others. Such conduct may be left for chemists like Mr. Brande, who, not being in the way of adding much of their own to the stock of science, might have some excuse for attempting to pilfer from their richer neighbours. But Sir H. Davy, who stands at the very top of the list of British discoverers; whose reputation is so high, and so deservedly high; whose inventive faculties are inexhaustible, has no occasion for such pitiful conduct. So far from supposing that I was doing him an injury by assigning the honour of the atomic theory to him who was really entitled to it, I never once doubted that he would himself admit the truth of my statement; and feel gratified for my supplying an omission which he himself on reflection must have wished he had not made —I mean the omission on his part of stating the origin of his notions on the subject.

When I stated that Davy's explanation of the atomic theory was not so perspicuous as that of Dalton, I meant merely that I did not understand it so well.

2. But "the full force of my hostility to Davy was exerted," it seems, "in depreciating the miners' safety lamp."—(Review, p. 122.)

Now I deny that I ever depreciated it. I did indeed, when I heard Davy's account of his first lamp read to the Royal Society, express my opinion in my Journal that it could not be used with safety. Whether this opinion was well or ill founded, I do not know. Perhaps it may have been ill founded. But as I honestly believed at the time that the lamp was hazardous, I think that I was bound to state my reasons for this opinion to the public. The lives of a great number of individuals were at stake. It was, therefore, important to point out every conceivable objection. It was Davy's business to examine these objections; to refute them if they were futile, and to benefit by them if well founded.

So far from supposing that I was injuring Davy, or endeavouring to detract from his merits, I conceived that I was doing him a service; and most persons in his situation would have been of the same opinion. How far my objections were well founded, it is not for me to say; but almost immediately afterwards Davy himself rejected his first lamp, and invented another, much superior to it in every respect.

Against this new lamp, I never in the *Annals of Philosophy* stated a single objection of my own, nor, as far as I recollect, of any other person. It is true indeed that a furious controversy respecting the person who had the merit of first inventing the miner's safety lamp, immediately arose, and various papers, written by the parties, were admitted into my journal. I acted with the utmost impartiality: as a proof of this, I may state that I received abundance of anonymous letters accusing me of partiality to Davy, to Stevenson, and to Clanny. I saw very early that the whole had become a party question, and that motives quite different from a regard to truth animated the disputants. The papers were inserted without any comment on my part; and as soon as I saw that they contained nothing but mutual recriminations, I stopped them altogether. One of the last, if not the very last, inserted was by Mr. Children. I happened to be in Cornwall when this paper was sent to my publisher. I had left materials for two successive numbers. The consequence was that Mr. Children's paper could not be inserted till after my return to London. When I reached home I found a letter from that gentleman complaining that his paper had been withheld from the public, and written in a style very different from what is usually to be found in a letter from one gentleman to another. Of this letter I took no notice. It gave me information for the first time, that Davy and his friends thought that I was hostile to his lamp.

My conduct then with regard to this controversy was fair and honourable. I was actuated by no hostility to Davy; but thought myself obliged to deal exactly the same justice to all claimants. That I discharged my duty as an editor with the most rigid impartiality appears from this, that all the controversialists accused me of partiality to their adversaries.

3. I am accused of garbling and disfiguring Davy's *researches on flame.* "The whole spirit of the original memoir has been dissipated. What remains is a mere *caput mortuum,* calculated to convey the most inadequate ideas of Sir H. Davy's discoveries."—(Review, p. 131.)

What answer can be given to this impudent assertion? To this paper of Davy I have devoted three pages, a greater space than is occupied by the account of potassium, or of the composition of muriatic acid, or indeed any topic discussed in the

System. Had I allowed three pages to every valuable paper which I had occasion to notice, my System would have extended to 100 instead of 4 volumes. The object which I had in view was to draw a comprehensive and distinct outline, and to leave the student to fill up the minute details by consulting the original papers, to which I always refer. I have given, I conceive, all the important additions to our knowledge of flame contained in Davy's paper. Had any thing been omitted, there can be no doubt that our Reviewer would have specified it. Since he has confined himself to general abuse, I may take it for granted that he had no particular omission to point out.

4. The fourth accusation is so very uncandid that I was surprised to meet with it even in this Review, virulent, and hostile, and malignant, as it is. In my short chapter on Electricity, which occupies only 10 pages, I state that in 1803, Berzelius and Hisinger made a capital discovery respecting the action of the galvanic battery in decomposing bodies. They found that oxygen and acids accumulate round the positive pole; while hydrogen, alkalies, earths, and metals, accumulate round the negative pole. Acids and bases may be made to pass through a considerable column of water, and even to cross each other, in order to accumulate round the poles to which they are respectively attracted. In the concluding paragraph I mention that Sir H. Davy took up the subject where Berzelius and Hisinger laid it down. His celebrated dissertation contains merely a verification of the law discovered by Berzelius and Hisinger. I then state his subsequent steps and discoveries.—(System, i. 171.)

On this statement of mine, the Reviewer descants in four long pages, and affirms in direct terms that the law in question was not discovered by Berzelius and Hisinger, but by Sir H. Davy. To this I answer, that I have quoted the very words of their paper. It was published in 1803 in German and French, and in 1806 in Swedish. It was never translated into English; but Davy quotes it in his celebrated lecture, and, therefore, was acquainted with it. My statement being true, I was bound as an honest man to make it; nor do I see that it takes in the least from the value of Davy's paper. The discovery of Berzelius and Hisinger remained neglected and unproductive, and might have so continued till the present day, had not Davy taken up the subject where they laid it down; and by his genius and industry, aided by a more fortunate situation, laid it open to all the world; and had he not by his subsequent discoveries awakened the attention of chemists to its great importance as an instrument of analysis.

The Reviewer mixes these unjust remarks with accusations against me for passing over Davy's electrical discoveries so slightly as I have done. Had he been candid enough to quote the two concluding sentences of my chapter, the true motive of my conduct would have appeared, and his animadversions would have been spared. These two sentences are as follows: "These and many other topics will find their place in another work, which I intend to publish hereafter, on Electricity and Galvanism. In the present work, I think, they would be improperly introduced, as they would divert our attention too long from the proper phenomena of Chemistry."—(System, i. 172.)

This work, for which I have made considerable preparations, would have appeared before this time, had my professional duties, which are very laborious, left me sufficient time to arrange it for the press. When it appears, our Reviewer will have another opportunity of displaying his talents for abuse.

5. I am accused of having ascribed the first accurate experiments on chlorine to Gay-Lussac and Thenard; though I was well aware that they had been made by Davy.

It is somewhat singular, and shows clearly the motives by which this writer was actuated, that his own statement proves to a demonstration that my account is correct. To be satisfied of this, the reader has only to turn his attention to the dates of the papers respectively published.

On the 15th of December, 1808, a paper was begun to be read to the Royal Society, by Sir H. Davy, entitled, "An Account of some new Analytical Researches on the Nature of certain Bodies." The reading of this paper occupied two evenings. In it Davy quotes repeatedly a number of the Moniteur for the 27th May, 1808, which contained an abstract of the experiments of Gay-Lussac and Thenard. He even mentions some of their attempts to decompose chlorine; though the most important of their experiments on muriatic acid and chlorine could not have been in that Moniteur, as they were not read to the Institute till the 27th February, 1809.

Now the eighth section of this paper contains Davy's researches and opinions concerning muriatic acid before he was aware of the experiments of Gay-Lussac and Thenard. He made various attempts to decompose muriatic acid without succeeding; and concluded from his experiments that muriatic acid gas, when as dry as it could be made, contained the third of its weight of water. After relating many attempts which he had made to decompose muriatic acid, and which, though unsuccessful, exhibited many curious and highly important results, he concludes in the following manner, which the Reviewer has misquoted:

"There is, however, much reason for supposing that in the singular phenomena of inflammation and detonation that have been described, the muriatic acid cannot have been entirely passive; and it does not seem unfair to infer, that the transfer of its oxygen, and the production of a novel substance, are connected with such effects, and that the highly inflammable nature of the new compounds partly depends upon this circumstance. I am still pursuing the inquiry, and I shall not fail immediately to communicate to the Society such results as may appear worthy of their attention."

There can be no doubt, from all that appears in this paper, that in the month of January, 1809, Davy was of opinion that muriatic acid is composed of oxygen and a combustible basis. The experiments of Gay-Lussac and Thenard were read to the Institute on the 27th of February, 1809. An abstract of them was published in the second volume of the Memoires d'Arcueil during the summer of 1809. Gay-Lussac and Thenard showed that muriatic acid cannot be obtained from chlorine except by means of hydrogen, or some substance containing it. They conclude their experiments in the following manner:

"Le gaz muriatique oxigené n'est pas, en effet, décomposé par le charbon, et on pourroit, d'apres ce fait et ceux qui sont rapportés dans ce Memoire, supposer que ce gaz est un corps simple. Les phénomènes qu'il présente s'expliquent assez bien dans cette hypothèse; nous ne chercherons point cependant à la défendre; parce qu'il nous semble qu'ils s'expliquent encore mieux en regardant l'acide muriatique oxigéné comme un corps composé."—(Mem. d'Arcueil, ii. 357.)

How was it possible after reading these two papers to avoid saying that the first great addition to our knowledge of chlorine was made by Gay-Lussac and Thenard? Davy attempted to decompose muriatic acid, but did not succeed. The French chemists showed that oxygen could not be extracted from chlorine by any method whatever, and they state in explicit terms that it might be considered as a simple body.

Davy's next paper, entitled "Researches on the Oxymuriatic Acid, its Nature, and Combinations," was read on the 12th of July, 1810. In the introduction to this paper he gives an historical detail of what had been done respecting the oxymuriatic acid, mentions the paper of Gay-Lussac and Thenard as already published, and states the curious nature of the experiments contained in it. It is clear then to a demonstration, that the experiments of these gentlemen were generally known before Davy suggested his opinion that chlorine is a simple body. This is all that I state in my System, and I never so much as dreamed that any person either would or could call the accuracy of the statement in question.

Davy, in the paper just mentioned, and in another read to the Royal Society on the 15th November, 1810, details the experiments which he had made to see whether chlorine gas could be decomposed, shows that they were all unsuccessful, and that we have no evidence whatever that it is a compound. Hence he deduces that, in the present state of our knowledge, we must consider it as an undecompounded substance. The present theory then, respecting chlorine and muriatic acid, is owing to the sagacity of Davy. This I have stated in my System in the very strongest terms, and have given Davy all that credit to which, in my opinion, he is fully entitled.

But I must now draw the reader's attention to another particular, because it shows that this malignant writer was conscious of the inaccuracy and falsehood of his statements, and that he drew them up with no other view than to make up the appearance of a case, by jumbling together the most monstrous and inconsistent falsehoods. In my account of the Improvements in Physical Science during the Year 1815, inserted in the first number of the seventh volume of the *Annals of Philosophy*, I notice (p. 27) the efforts of the French chemists to deprive Davy of the honour of this discovery, and show their futility and absurdity. These remarks are terminated by the following observations: "If Gay-Lussac always maintained it, as he informs us, but was prevented from publicly embracing it by the authority of Berthollet, we may pity his pusillanimity, but cannot on that account admit his claim as the first propagator of a theory which he publicly opposed." (P. 28.) The commentary on this passage by the Reviewer is as follows: "Since that period, however, Dr. Thomson has set up as the autocrat of chemistry, assigning to each of his contemporaries the rank he ought to occupy with despotic decision. Of Gay-Lussac he says, 'we may pity his pusillanimity.'"—(Review, p. 123.) Had the Reviewer quoted the passage fairly, the absurdity of this tirade would not only have been obvious to every reader; but it would have appeared (contrary to his assertions) that, so far from having attempted to deprive Davy of the honour of being the author of the modern theory respecting muriatic acid and chlorine, I have done him the most ample justice.

6. I am accused of having perverted Davy's account of chloriodic acid to suit my own atomic notions.—(Review, p. 142.)

I request the reader to peruse my account of this substance in vol. i. p. 194, of the System of Chemistry. If I have not stated Davy's experiments without any perversion, I am no judge of what perversion means.

7. But one of the most curious, as well as uncandid, attacks upon me by the Reviewer is contained in his observations respecting the composition of phosphoric acid. The passage is too long to quote it here: I must, therefore, refer the reader to it in pages 147 and 148 of the Review. I have already, in various articles in the *Annals of Philosophy*, given an historical sketch of the facts respecting the discovery of the composition of phosphoric acid; but in order to show the reader the malignity as well as falsehood of the Reviewer's account, I must give a short view of these facts here.

The first attempt to determine the constituents of phosphoric acid was by Lavoisier (Mem. Paris, 1777, p. 65; 1780, p. 343; and 1783, p. 416). His experiments were continued 12 years, and his ultimate result was, that the acid is composed of two phosphorus and three oxygen.

Davy, in his Elements of Chemical Philosophy (p. 286), says, that when a grain of phosphorus is strongly heated in oxygen gas, it absorbs four cubic inches and a half of the gas. This statement coincides with the original one of Lavoisier. Whether it was the result of experiment on the part of Davy does not appear.

M. Rose endeavoured to determine the question in 1806 (Gehlen's Jour. ii. 309, Second Series), by acidifying phosphorus by means of nitric acid, and then saturating the acid with oxide of lead. He deduced from his experiments that the acid is a compound of 100 phosphorus + 114·75 oxygen: 50 grains of phosphorus yielded, when thus treated, 481 grains of phosphate of lead. But as phosphate of lead is composed of 14 oxide of lead + 3·5 phosphoric acid, it is obvious that 481 grains of it contain only 96·2 grains of phosphoric acid. Hence the true deduction from Rose's experiments is, that phosphoric acid is a compound of 100 phosphorus + 92·4 oxygen.

In the year 1816, a paper of mine on the composition of phosphoric acid, and on various phosphates, was read before the Royal Society. I had repeated Rose's experiments with great care, but found the method not to be relied on. I, therefore, drew my conclusions from the gas absorbed, when a given weight of phosphorus is burnt in retorts filled with common air. The result of my trials was that one grain of phosphorus, when thus burnt, absorbs 3·66 cubic inches of oxygen gas. Hence I concluded that the acid is composed of 100 phosphorus + 123·5 oxygen. This result was not quite correct. The error amounted to nearly 1-13th of the whole, or about 1-4th of a cubic inch, by which the oxygen gas absorbed was too small. The error was owing to the extreme difficulty of burning all the phosphorus, or of weighing with accuracy the unburnt portion.

Soon after this, a set of experiments on the same subject was published by M. Dulong. His result almost agreed with mine. According to him, phosphoric acid is composed of 100 phosphorus + 124·8 oxygen.

A few months later, Berzelius favoured the chemical world with the result of his own experiments on the subject. He found the acid composed of 100 phosphorus + 128·17 oxygen.

I do not see any reason why I should be ashamed of my experiments. Compared with the preceding statements of Lavoisier, Davy, and Rose, they are exceedingly accurate. And though Dulong and Berzelius, who followed me, have approached somewhat nearer the truth, yet they were unable to reach it.

The principal object of my paper was to show that one of

Berzelius's *canons*, which he still employs in all his reasonings, is not so general as he had supposed. This canon is as follows: "In all salts, the oxygen contained in the acid is either equal to that contained in the base, or it is a multiple of it." I showed in my paper that this supposed canon was inconsistent with the composition of several of the phosphates. My reason for withdrawing this paper was, that after it had been read I had made the experiments on phosphuretted hydrogen gas (which I afterwards published), and from which I deduced that phosphoric acid is composed of 100 phosphorus + 133$\frac{1}{3}$ oxygen. This discovery made it necessary to alter all the analytical results, because they had been calculated from incorrect data. Indeed it became evident that a repetition of the analyses would be necessary to ensure precision; and to oppose Berzelius's canon with inaccurate experiments would have been both imprudent and useless.

My experiments on phosphuretted hydrogen gas were made with so much care that I confided in the accuracy of my results; but I was unable to reconcile them with Berzelius's analyses of the phosphates; nor was I able to prove in a satisfactory way that Berzelius was wrong.

While I was reflecting on this want of coincidence, and trying to account for it, I received a short paper from Mr. Dalton, which I published in the *Annals of Philosophy*, xi. 7. He informed me that he had repeated my experiments on phosphuretted hydrogen gas, and had found that it combined with twice its volume of oxygen gas. The reader ought to be informed that Mr. Dalton had previously made experiments on this gas, and had found that it combined with its own volume, or with 1$\frac{1}{2}$ its volume of oxygen. I had every reason, therefore, to confide in the accuracy of his new statement; and I adopted it the more readily, because it enabled me to reconcile Berzelius's analyses with my own experiments. The sheet of the fifth edition of my System containing my account of phosphorus, was in the press, and I altered the numbers in it so as to bring the composition of phosphoric acid to agree with the statement of Dalton and the analyses of Berzelius. My own experiments I applied to hypophosphorous and phosphorous acids. I represented the composition of the three acids as follows:

	Phosphorus.	Oxygen.
Hypophosphorous acid.	1 volume	+ $\frac{1}{2}$ volume
Phosphorous acid.	1	+ 1
Phosphoric acid	1	+ 1$\frac{1}{2}$

Thus my own experiments, those of Dalton, and those of Berzelius, all tallied with each other. The coincidence was irresistibly seducing. I was constrained to yield to it.

Soon after this, I went to Glasgow, and I was not in possession of a laboratory for nearly two years. One of the first things which I did, as soon as it was in my power, was to repeat Dalton's experiment. I found it inaccurate. The whole structure immediately tumbled to the ground; and I was led back to the original opinion which I had stated in my paper on phosphuretted hydrogen gas. And those gentlemen who attended my lectures the ensuing course will remember that I then gave the composition of phosphorous and phosphoric acids precisely as in my sixth edition.

Davy's paper appeared soon after, and confirmed me in the accuracy of my experiments. Still I was unable to reconcile the analyses of Berzelius with this view of the composition of these acids, and this induced me to express myself with some reserve in the account of the composition of this acid which I gave in that edition; and I am of opinion (whatever the Reviewer may say to the contrary) that such reserve and hesitation ought always to be met with in elementary books, unless we can clearly show that the results stated by one of the parties are erroneous.

It was only after I had made the experiments related in my paper, which begins the New Series of the *Annals*, for January, 1821, that I was able to show that Berzelius's analysis of phosphate of lime is inaccurate. These experiments were necessary before the subject could be considered as closed.

My conduct during the whole of this discussion has been, I think, just what it ought to have been. The hesitation and uncertainty in which I remained till I obtained decisive evidence, ought rather I think to be mentioned in my praise than as a proof of want of consideration.

Thus have I minutely examined all the accusations of the Reviewer, which affect my character; and I appeal to the candour of the reader if they have not been shown to be every one of them false, malignant, and disgraceful, to the accuser.

As for my observations on the Council of the Royal Society, to which the Reviewer alludes in so petulant a manner, I have only to say that when I made them, I thought them just, and I still continue of the same opinion. As a Fellow of the Society, I thought myself not merely entitled, but called upon, to notice any little inadvertence on the part of the Council of the Society. I have reason to know that some of the gentlemen who were members of the Council at the time, whom I have the happiness to reckon among my friends, were not in the least hurt at what I said. One gentleman indeed told me that he was displeased, but he was not a member of the Council; and I never have been in the habit of regulating my conduct by his particular taste.

III. *Errors from Ignorance.*

This part of the Review, had it been drawn up by a man of skill and candour, might have been valuable. It is scarcely possible for one practical chemist to review the labours of another without throwing out remarks which may benefit the reader. Every experimenter has methods of his own, which he has brought to a considerable degree of perfection, though they have probably been overlooked by his fellow labourers. The very circumstance of drawing the attention of chemists to such particulars cannot but improve the art. Nor is it less advantageous to compare together the methods followed by different chemists to accomplish the same object. I was sorry to observe nothing of that nature in this part of the Review. The most consummate petulance, accompanied as it always is with the most woful ignorance, characterizes every one of the Reviewer's observations. To enter into a minute refutation of such accusations would be a superfluous task. To the real chemist, their absurdity will appear at a glance; and those who are not acquainted with the subject are not likely to trouble themselves either with the accusations or the answers. I think it necessary, however, though at the risk of encroaching upon the patience of the reader, to notice every accusation which appears to be of any importance.

1. *Light*.—The attack upon my account of *light* in p. 128 of the Review cannot surely require any answer. I am not aware of any thing wrong in my observations, nor do I admit the justice of a single statement advanced by the Reviewer in opposition to them. One specimen of the Reviewer's mode of writing will be amply sufficient for the reader. I say that the "particles of light repel each other, while the particles of other bodies attract each other, and accordingly are found cohering together in masses of more or less magnitude."—(System, vol. i. p. 23.) To this the Reviewer subjoins: "This is sad prosing. Have the sun and stars no sensible magnitude? Do the particles of gaseous bodies cohere together?" (Review, p. 128.) It would appear from this passage, that, in the Reviewer's opinion, the sun and stars are mere masses of light. Unless he thinks so, his observations are absurd and inapplicable; and if he does think so, he is a very fit person truly to ridicule the opinions of others respecting light! The particles of gases do not cohere together, because they repel each other as well as the particles of light. But I should like to know the gaseous body which does not enter as a constituent into some solid or fluid body, whose particles cohere. Have we any evidence that light constitutes a ponderable part of any body? Such is the Reviewer's mode of throwing ridicule on my account of light! It demonstrates, I think, that the chapter contains nothing upon which he could fix any animadversions; for could he have pointed out any thing really absurd or inaccurate, the passage about the sun and stars would surely have been omitted.

2. *Expansion*.—The Reviewer's observations respecting expansion in p. 130 of the Review, show merely that he has not

considered the mechanism of expansion; and that ignorance of a subject does not deter him from writing on it.

I cannot avoid noticing a most extraordinary passage in the same page of the Review. A paper exhibiting the strength of alkalies, drawn up by Mr. Charles Tennant, of St. Rollocks, Glasgow, is inserted in the *Annals of Philosophy*, vol. x. p. 115, with the author's name attached to it. This paper the Reviewer ridicules in such a way that every reader must suppose that I was the author of it. Now, in the first place, the table is an accurate one; and Mr. Tennant, who drew it up, is entitled to great credit for his sagacity; for he had deduced the true atomic weights of potash and soda (6 and 4) from his own experiments several years before they had been recognised by any scientific chemist. It was this circumstance which drew my attention, and induced me to insert the paper, in order to secure for Mr. Tennant that priority to which he was justly entitled. The table was intended for the bleachers only, and I have had the means of knowing that it has been of great use to them. But supposing that the paper had been trifling and useless, what had that to do with my System of Chemistry? Have no trifling papers appeared in the Journal of the Royal Institution?

3. *Thermometer.*—The Reviewer affirms that my account of this instrument is contemptible. I have had the curiosity to look into Mr. Brande's account of it, and find it nearly the same as mine. It certainly contains nothing more than mine does. The Reviewer conceives that I should have given an account of the mode of making these instruments. If he will come and attend a course of my lectures in the University of Glasgow, he will have an opportunity of hearing a minute detail of every thing that I know respecting the making and use of this very important instrument. But such details would have been inconsistent with the nature of my System of Chemistry. I might as well have given a minute account of the mode of making crucibles, retorts, mirrors, air-pumps, electrical machines, hammers, anvils, and stone jugs. I might in this way have swelled out my book to the size of an Encyclopædia; but I should have rendered it much less adapted to the student of chemistry than it is.

4. *Galvanic Battery.*—I have "repeated the gross blunder which, in our former critique, we pointed out relative to the energy of the pile, which he very ignorantly says, 'at least as far as chemical phenomena are concerned, "increases in proportion to the size of the pieces.'"—(Review, p. 136.) That this statement was not the effect of ignorance must be known to all who have attended my lectures in Glasgow. I have an apparatus for the purpose of showing that the heat, and consequently the action of the pile upon metals, depends upon the size of the plates; and that thick wires may be melted by a battery which neither gives shocks, nor decomposes water, nor ignites char-

coal. It was the heat and effect on metals to which I alluded under the name *chemical effects;* but the phrase, it appears, from the Reviewer mistaking it, was not sufficiently precise.

5. I never mention chlorate of potash nor red oxide of mercury as convenient substances for furnishing oxygen.—(Review, p. 140.) Had this statement been true, I do not think the omission would have been of any consequence. But in vol. ii. p. 232, of my System, I expressly say that 100 parts of chlorate of potash, when heated, give out 38·69 parts of oxygen.

I never employ these bodies myself for procuring oxygen gas, because I can obtain it equally pure, and at a much smaller expense, from the black oxide of manganese. If the oxygen gas given out about the middle of the process be collected, it will be found very pure. I obtain it every year by this process with less than a half per cent. of azote.

6. What am I to make of the following quotation from my System? "The weight of an atom of oxygen in the subsequent part of this work will be denoted by 1st, a volume of oxygen is equivalent to two atoms, provided we suppose, as I have done, that water is a compound of one atom of oxygen and one atom of hydrogen."—(Review, p. 140.) The passage in my System is really as follows: "The weight of an atom of oxygen in the subsequent part of this work will be denoted by 1. A volume of oxygen is equivalent to two atoms, provided we suppose, as I have done, that water is a compound of one atom of oxygen and one atom of hydrogen."—(System, i. 179.) The Reviewer must have been sadly put to it in his search into mistakes, when he was driven to the necessity of creating them by misquotation, that he might have an opportunity of animadverting upon absurdities which originated with himself.

7. I quote the following passage from the Review, without pretending to understand it. "The deutoxide of chlorine was discovered about the same time by Sir H. Davy and Count Von Stadion, of Vienna; but Davy's account was published sooner than that of Count Von Stadion." The account of the former was published in Thomson's *Annals* eight months before that of the latter appeared. Surely some qualm of conscience must have smitten our compiler in writing his next page. "But the properties of the substance described by the Count differ so much from those of the gas examined by Davy, that it is probable they are distinct substances."—(Review, p. 142.)

Does the Reviewer mean to arraign the accuracy of my statement respecting the experiments of Count Von Stadion? It looks a little like it. Davy's paper was read to the Royal Society in May, 1815, and published about the end of July. The Count's paper appeared in Gilbert's Annalen for Feb. 1816, or about six months later than Davy's. My reason for believing that Von Stadion was unacquainted with Davy's paper was not merely because the Count no where alludes to it, but because

Gilbert, in his appendix to the Count's paper, in which he gives an historical detail of all that had been previously done on the subject, never alludes to Davy's paper. He must, therefore, have been unacquainted with it in February, 1816.

8. In p. 142, the Reviewer ridicules me for stating that a volume of chlorine is equivalent to an atom, while half a volume of oxygen is equivalent to an atom. These inconsistencies, it seems, I should have avoided had I followed Davy. I must bear the ridicule as well as I can, because I am satisfied that my statement is true, and that it is of importance.

9. The Reviewer has pointed out an arithmetical error in my section on *fluorine;* and asks how I could have allowed it to remain uncorrected in two editions of my System. The reason was simply that I did not suspect its existence. In my fifth edition I had deduced the atomic weights of bodies as nearly as I could from the experiments of others. I had formed the resolution of investigating the subject with all the precision of which I was capable by experiments of my own. Hence in the sixth edition I naturally allowed every thing which I had not yet verified by experiment to remain unaltered. This was the reason why the section on fluorine was printed verbatim from the fifth edition.

The readers of the *Annals of Philosophy* are aware of the assiduity with which I have prosecuted this investigation. I have now determined the atomic weights of all the simple bodies, except about 15. Fluoric acid has occupied my attention as well as the others; but my experiments on it are not yet ready for publication. I may, however, state here, that at present I am inclined to consider fluor spar as a compound of

Lime.	3·5
Acid.	1·25

Hence the atomic weight of the acid seems to be 1·25. It may be a compound of one atom of oxygen and one atom of an unknown combustible, whose atomic weight is 0·25, or double that of hydrogen. If we suppose it a compound of equal volumes of fluorine and hydrogen, then the atom of fluorine will weigh exactly as much as the atom of oxygen. This is rather against the probability of the existence of fluorine.

10. In p. 144 of the Review, the writer thinks proper to dispute the accuracy of my experiments on hydrocarbonic oxide, though it is evident that he has never repeated them. I have prepared this gas at least a dozen of times; have exhibited its properties to my students; and it has been repeatedly examined and analyzed by my pupils.

11. In the same page, the Reviewer denies that I ascertained in 1810 that chloric ether is a compound of olefiant gas and chlorine. I refer the reader to my paper on the subject printed in the first volume of the Wernerian Transactions, p. 516. He

will find a set of experiments on it detailed, and my conclusion from them, as follows: "It is a substance of a nature quite peculiar, and seems to consist of the two gases simply combined together." I did not determine the proportions in which the two gases unite; that was reserved for Colin and Robiquet; but my experiments left me in no doubt respecting the constituents.

"In the same paper we find him using for another analysis an olefiant gas, which, by his own account, contained 16 per cent. of common air, and the oxygen gas was mixed with 11 per cent. of common air. We would like to know how he ascertained so precisely the proportion of common air when he was in the habit of operating with such impure materials."—(Review, p. 144.)

I shall gratify this laudable curiosity of the Reviewer. I ascertained the volume of oxygen gas in the olefiant gas by means of nitrous gas, employing Dalton's formula for the purpose. This volume, when found, I multiplied by 5, and considered the product as the volume of common air mixed with the olefiant gas. I ascertained by repeated trials the volume of pure hydrogen, which left the smallest residue when detonated with the oxygen gas. The pure oxygen was obtained by taking half the volume of this hydrogen gas. The residual volume I considered as azote. To it I added one-fourth of its volume, and called the sum the volume of common air present. Oxygen containing 1-11th of its bulk of common air is far from impure. It contains about 92 per cent. of pure oxygen, and only 8 per cent. of azote. With such a gas, very good results may be obtained. Indeed in many cases it is expedient to diminish the purity of the oxygen gas employed, by mixing it with a certain portion of common air.

12. But we come now to what the Reviewer seems to have considered as his master-piece,—his defence of Mr. Brande's notion, that carburetted hydrogen gas is merely a mixture of olefiant gas and hydrogen gas. By this time I dare say he is ashamed of what he has written on this subject, and would willingly barter nine-tenths of his wit for one-tenth of my precision. For Dr. Henry's paper published last summer in the Philosophical Transactions has demonstrated the peculiar nature of this gas, if any demonstration was necessary. Indeed my statement in the *Annals of Philosophy*, xvi. 380, was decisive of the inaccuracy of Brande's views. That a mechanical mixture of three volumes of olefiant gas and two volumes of hydrogen should be uniformly extricated from stagnant water in all places would be truly miraculous. Dr. Henry found its specific gravity the same as I had done, and that specific gravity is incompatible with Brande's notion, as are indeed all the properties of the gas. The Reviewer has mis-stated my reasoning in the *Annals;* but it is not worth while to put him right. If Mr. Brande chooses to persist in his opinion in the face of common sense, I have

nothing to say to the contrary. Let my arguments and the experiments of Dr. Henry on the one hand, and the statements of Mr. Brande and the witty observations of the Reviewer on the other, be placed in opposition to each other, and let the chemical world judge between them.

13. With respect to my mode of taking the specific gravity of phosphuretted hydrogen gas, the Reviewer observes: "This confession betrays poverty of invention, and ignorance of the methods previously practised in such cases."—(Review, p. 147.) This observation from an individual, who, so far as is known to the public, never took the specific gravity of a gas in his life, and directed against me, who have determined the specific gravity of more than 20 gases, with a degree of care and accuracy seldom equalled, and never surpassed, had surely been better spared. I affirm that my method is susceptible of greater accuracy than that which the Reviewer insinuates that I did not know; and this I affirm from having repeatedly tried both methods. The less complicated an experiment is, the greater is the chance of accuracy.

14. In page 149 of the Review, a most indecent attack is made upon a paper of mine printed in Nicholson's Journal, vol. vi. p. 92, "On the Compounds of Sulphur and Oxygen." This paper was printed so carelessly that the meaning in several places is much obscured. The experiments described in it were made with an apparatus by no means well adapted for accuracy; but they were so often repeated, and so carefully, that the errors committed are inconsiderable. In my calculations, I employed Mr. Chenevix's analysis of sulphate of barytes as a datum, which is now known to be inaccurate. When the error thence arising, and which has nothing to do with my experiments, is corrected (and it is in any one's power to make the correction), the experiments related in pages 95 and 96 show that sulphurous acid is composed of

Sulphur	48·17
Oxygen	51·83
	100·00

Now the true composition of the acid is now known to be:

Sulphur	50
Oxygen	50
	100

Will the Reviewer pretend that this result is not near enough the truth to warrant what I have said in my System? Let him compare it with the latest analysis of Berzelius, and see whether it will suffer by the comparison. As for the experiments of Davy, to which the Reviewer refers as the first which established the true composition of sulphurous acid, I am not acquainted with them.

15. I may leave the Reviewer's remarks on my account of arsenic to any reader of the least candour without being apprehensive as to the result. My statements in the System (as the reader will find) were deductions from the experiments of Berzelius, which I have since found not to be quite accurate. The true atomic weights of arsenic and its compounds are given in my paper in the *Annals of Philosophy* (New Series), vol. i. p. 13; and vol. ii. p. 129.

16. The Reviewer affirms that the analyses of Vauquelin, Arvedson, and Gmelin, all concur to show that the weight of an atom of lithia is 2·3, and that I, in defiance of these authorities, make it 2·25.—(Review, p. 150.) I have shown in the very page to which the Reviewer alludes, that the mean of the experiments of Vauquelin and Arvedson give the weight 2·254, which very nearly agrees with my number.

17. The remark upon my directions for forming muriate of barytes (Review, p. 150) shows that the Reviewer is not practically acquainted with the making of this salt. I have made it very often, and have tried the Reviewer's method as well as others. The directions in my System I think the best. Nothing is gained by keeping out the iron of which the author speaks; because the muriatic acid of commerce is never free from that metal. You must, therefore, heat your salt, and redissolve it in water and crystallize, before you can get it in a state of purity, whatever care you take to exclude the iron mixed with your barytes. During the last two years I have generally prepared the salts of barytes by fusing the sulphate with an alkaline carbonate, washing off the alkali, and then dissolving the carbonate of barytes in the required acid. I am not sure that this method is more economical than the other, but it is less troublesome.

18. The absurdities respecting manganese to which the Reviewer alludes (p. 150) are absurdities of his own, not of mine. I leave him to correct them at his leisure. As for my account of steel, which he says has long been the ridicule of practical men (p. 150), I have only to say that I have given the best account of it which I could. I have witnessed the process several times both in England and Scotland, and have availed myself of the description of it published by Mr. Collier in the fifth volume of the Manchester Memoirs. I must rest contented in my ignorance till Mr. Brande thinks proper to enlighten the world on the subject.

19. The sneer against me (Review, p. 151) for not adopting Mr. Donovan's estimate of the composition of the mercurial oxides is quite misplaced. When I praised Mr. Donovan's

paper, I did not allude to these estimates. I can make allowance for errors even in a good paper. If this were not made, what chemical paper would be entitled to praise? Where is the chemist to be found who has not very frequently fallen into analytical mistakes?

20. The sneers at my account of ammonia could not have come from any writer of the smallest candour. I do not attempt to reconcile the discordant statements. My own of course are those which I consider as nearest the truth. Even they would require to be re-examined. The experiments were made 20 years ago, when neither my weights nor my measures were so accurate as they are at present.

21. The Reviewer is quite indignant that I have given Mr. Dalton's table of the strength of sulphuric acid, instead of Dr. Ure's.—(P. 153.) I consider Dalton's, as far as it goes, as the best of the two. To a practical chemist, such a table is in fact of very little use. I find it of none, except in the rare case of having a dilute acid by me. My method of proceeding is this: I keep by me a few pounds of sulphuric acid, which I have purified by distillation, and concentrated as far as possible. Such acid has the specific gravity 1·8447, and is composed of 5 real acid and 1·125 water; consequently 6$\frac{1}{8}$ grains of it contain exactly 5 grains of true acid. I can weigh the exact quantity of this acid wanted with as little trouble as of diluted acid; I afterwards dilute this portion at pleasure. Indeed I have a glass measure graduated to grains, by which I can measure the quantity of acid when minute precision is not wanted. A table similar to that of Dr. Ure's, we find of no use in my laboratory.

22. The sneer about lampic acid (p. 153) had better have been omitted, Mr. Daniell having himself acknowledged that this acid is merely the acetic, and consequently verified my opinion.

23. I do not believe that in the whole history of chemistry any thing can be pointed out more uncandid or unjust than the Reviewer's remarks upon my paper on Oxalic Acid, published in the Philosophical Transactions for 1807. I did not succeed in ascertaining the exact proportion of water contained in this acid, my method (simply heating the acid on a sand-bath) not being capable of separating the whole water. The consequence of this was, that my oxalic acid contained water, and of course my data for determining the composition of the oxalates being wrong, their composition is inaccurately stated in my paper. To correct the error it would be necessary to subtract the portion of water which I allowed to remain in the acid. If the Reviewer does so, he will find my results tolerable approximations.

The value of the paper does not depend upon these numerical analyses. It contains a great deal of matter, of which I have uniformly availed myself in all the editions of my System published since 1807. I may mention here that Berard wrote a paper on the same subject soon after me, and on purpose to rectify my analyses; but his results deviate as far from the truth as mine, and obviously from the same cause.

The allegation by the Reviewer, that my analysis of oxalic acid was inaccurate because my oxalic acid still retained 32·5 per cent. of water is disgracefully unjust. He must have known that my experiments were not made upon oxalic acid, but upon a dry oxalate. No water existed in the acid as I employed it; and, therefore, none was to be deduced. My near approach to truth in these experiments, notwithstanding the numerous difficulties attending my method, afford unequivocal evidence of the great care employed in the experiment.

I may mention here that Berard in his paper denied the existence of binoxalate of strontian. I have frequently formed this salt since, as well as binoxalate of barytes. They are both crystallizable and well defined salts.

24. The Reviewer's observations on my analysis of chloride of lime (p. 156) are so ridiculously absurd that it would be waste of time to make a serious answer to them. Does he know so little of this substance as to suppose that chlorate of lime ever does or can enter as a constituent into it? If he thinks so, I would advise him to try a few experiments on the passage of chlorine gas through dry lime. They would cure his petulance, and give him some information on a subject about which he obviously knows nothing.

25. The Reviewer's remarks about the quantity of muriatic acid gas absorbed by water (p. 157) are as usual very witty; but the wit does not affect me. I have given the result of my experiments. Let him repeat them, and show them to be inaccurate, and then sneer away and welcome. Till then I shall only say, that it is easier to sneer than to experiment.

26. The Reviewer's remarks on my account of the mode of preparing chlorocyanic acid are shamefully unjust.

27. I have now noticed all the Reviewer's attacks upon me for want of knowledge, which seem entitled to any observation, with the exception of two, which I cannot with propriety pass over in silence. I must still, therefore, request the reader's indulgence for a short time before I conclude. I shall first quote the following paragraph from the Review:

"Mineralogy, which now begins to assume the systematic aspect of the other parts of natural history, by the labours of Werner, Haüy, Mohs, and Jameson, is here exhibited in a truly chaotic state. He has no allusion whatever to the natural history method of Mohs, which promises to do for the study of minerals what the sexual system did for plants; enabling a person on taking up a specimen to refer it to its peculiar class, order, genus, and species, till he discovers its name and various relations. His first chapter "On the Description of Minerals," is copied from Prof. Jameson's Treatise on the External Characters. We find the same chapter, in the same words, in the former edition, but with a reference to Mr. Jameson, which is now suppressed. The only observable alteration, indeed, in his present article on Mineralogy, is the erasure of Prof. Jameson's name wherever it formerly occurred."—(Review, p. 166.)

The arrangement of minerals which I have adopted is that of Werner, with a few slight alterations to fit it better for a chemical work. If it be a chaos, then the same term may be applied to every system of mineralogy which has hitherto appeared. How far the Reviewer's statement, that the Mineralogy in my sixth edition is just a reprint of that in the fifth, the reader will be enabled to judge when I inform him that it contains no fewer than 38 new species, the names of which I shall here subjoin for the Reviewer's satisfaction:

Turquoise,
Peliom,
Colophonite,
Helvine,
Eudyalite,
Allophanite,
Basalt jasper,
Spherulite,
Karpholite,
Mesolite,
Skolezite,
Petalite,
Gieseckite,
Ambligonite,
Caranthine,
Calamite,
Baikalite,
Fassaite,
Polyhalite,
Bucholzite,
Papercoal,
Eucairite,
Tennantite,
Seleniuret of copper,
Bismuthic carbonate of copper,
Titaneous iron ore,
Sulpho-arseniate of iron,
Skoroclite,
Knebelite,
Carbo-silicate of manganese,
Glance nickel,
Wodan pyrites,
Arseniate of nickel,
Antimonial sulphuret of lead,
Arsenio-sulphuret of lead,
Antimonial arseniate of lead,
Tungstate of lead,
Aluminate of lead.

Besides these new species, which constitute no trifling addition, I have changed the position of a considerable number of species, rectified the description of more than two-thirds of the whole, and added many new analyses, all indeed that had come to my knowledge. I conceive, therefore, that the attention which I paid to this part of the work, and the improvements introduced into it, are much more considerable than could have been anticipated. Instead of censure, therefore, I was entitled to no small degree of praise.

With respect to the system of Mohs, which has been adopted by Jameson in his last edition, I must confess myself an incompetent judge, because I do not understand it. I have perused Mohs' little treatise on the Characters of the Classes, Orders, Genera, and Species, a copy of which the author did me the honour to present to me. I have likewise read the account of the method published in the Edinburgh Journal; but neither of these accounts puts it in my power to understand the nature of the arrangement. Mr. Jameson's last edition is a cypher without a key. Under these circumstances, I thought myself obliged to omit my references to Jameson's System. I could not refer to the old edition after the author had published a new one; and I could not refer to the new edition, because I did not understand it. Thus circumstanced, I thought the best thing I could do was to refer to Hoffman's Mineralogy, instead of Jameson's. It contains the Wernerian descriptions in the very words of Werner; and is the original from which most of Jameson's descriptions are taken.

But even if I had understood Mohs's system, I question if I should have adopted it, because it is not adapted to a chemical view of minerals. If Mohs succeed in establishing an artificial system which will enable the student to find out the name of any mineral of which he may happen to have a specimen, he will perform a very useful task. But surely it will not be said that the last edition of Jameson's Mineralogy is in this predicament.

In a chemical treatise, the composition of minerals is the most important point. Now Mohs has not paid any attention to the composition of minerals, but has been guided by something respecting the crystalline shape which he has not put it in our power to understand. I shall give one order as an example, and I select it the rather because all the minerals arranged in it are chemical compounds.

ORDER VI.—BARYTE.

GENUS I.—*Lead Spar.*

Sp. 1. Sulphate of lead,
2. Molybdate of lead,
3. Chromate of lead,
4. Phosphate of lead,
5. Carbonate of lead.

GENUS II.—*Hal-Boryl.*

Sp. 1. Carbonate of barytes,
2. Sulphate of barytes,
3. Carbonate of strontian,
4. Sulphate of strontian,

GENUS III.—*Tungsten.*

Sp. 1. Tungstate of lime.

GENUS IV.—*Calamine.*

Sp. 1. Silicate of zinc,
2. Anhydrous carbonate of zinc,
3. Hydrous carbonate of zinc.

Genus V.—*Red Manganese.*

Sp. 1. Silicate of manganese.

Genus VI.—*Sparry Iron.*

Sp. 1. Carbonate of iron.

Now I appeal to every reader whether a chemist could adopt such an arrangement. All minerals are probably saline compounds, and Berzelius has gone a considerable way to prove this; but I do not consider mineralogy as yet ripe for a true chemical arrrangement. Much labour must still be bestowed upon the chemical analysis. I have been occupied with the zeolites occasionally for more than a year, and the 14 or 15 minerals which I have already analyzed have given me much additional information—enough to enable me to arrange my own cabinet; but not to publish a systematic arrangement of even the zeolites. I would still allow the Wernerian arrangement to remain, were I to publish a new edition of my System to-morrow. By the united exertions of chemists in every part of the world, a true natural arrangement of minerals will, by degrees, be accomplished. But *festina lente* is an excellent adage, of which the Reviewer would do well to avail himself.

The only other point to which I think it will be necessary to allude is, the animadversions of the Reviewer on my chapter on the analysis of minerals. He has pointed out one or two typographical errors, and invented as many more. This chapter was written nearly 20 years ago, and consists chiefly of specimens of the mode of analyzing minerals taken from the best analysts. Since that time I have repeated more than once almost every analysis contained in it. I could certainly have improved it somewhat; but to have given accurate formulæ for the analysis of every mineral is still beyond our power. A precise knowledge of the atomic weight of every constituent of the mineral kingdom is an essential preliminary. This I have been acquiring only since my sixth edition was published. Even at present my knowledge is incomplete, some very essential atomic numbers being still wanting. We lie under very great obligations to Klaproth, and Bucholz, and Vauquelin, for their numerous analyses of minerals. They have greatly enlarged our knowledge of the mineral kingdom; while they have invented many methods of analysis, which have added much to the resources of the practical chemist. But we still require more accurate methods than those with which these chemists were satisfied, before we can acquire that precise knowledge of the composition of minerals which is requisite for an accurate chemical arrangement. Towards this desirable object, I have turned a great deal of my attention, and have contrived a variety of methods for bringing the art of analysis to the requisite degree of perfection. Some of these I have occasionally published; and I am not without hope of being able, in the course of time, to lay a practical system of chemistry before the public. One part of this system indeed, that which respects the gases, I have nearly completed. But salts, metals, and minerals, present so very extensive a field, that I am apprehensive lest the life of one individual should be too short to traverse the whole of it. I take this opportunity of calling the attention of chemists to the importance of such a desirable object. The joint labour of many may accomplish with ease what would surpass the most gigantic efforts of a single individual.

Thus have I finished my remarks on Mr. Brande's Review of my System of Chemistry. When I perused it for the first time with attention in the month of February last, the impression which it left upon my mind was, that many of the animadversions must be well founded. They are made with an air of such confidence and plausibility that they are well calculated to make an impression on the reader. After having thus investigated them one by one, I am amazed to find how very few of them have any justice in them, and feel fully confident that every reader will participate in my astonishment, and agree with me that a more uncandid review has scarcely ever appeared, and that it fixes an indelible stigma both on the editor and the author.

ARTICLE II.

Astronomical Observations, 1822.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Feb. 23.	Emersion of Jupiter's third satellite	7h	7′	13″	Mean Time at Bushey.
		7	8	34	Mean Time at Greenwich.
Feb. 27.	Immersion of a small star per moon	8	31	49	Mean Time at Bushey.
Feb. 27.	Immersion of a small star per moon	8	53	21	Mean Time at Bushey.
Mar. 1.	Emersion of Jupiter's first satellite	6	56	27	Mean Time at Bushey.
		6	57	48	Mean Time at Greenwich.
Mar. 2.	Emersion of Jupiter's second satellite	6	35	33	Mean Time at Bushey.
		6	36	54	Mean Time at Greenwich.

The occultations of the stars by the moon were made under very favourable circumstances, and their brilliancy remained undiminished till obscured by the dark limb of the moon.

Article III.

Experiments and Observations on the Resistance of Water, with Remarks on the Apparatus. By Col. Beaufoy, FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Bushey Heath, March* 4, 1822.

In the third volume of the *Annals of Philosophy,* Dr. Thomson published several of the experiments made at Greenland Dock, between the years 1793 and 1798, on the resistance of water to variously shaped bodies. Among these were the direct and oblique resistance of two square iron planes, whose united surfaces measured 2·972 superficial feet, the centre of each plane being immersed three feet beneath the surface of the water. On reference to the original papers, I am persuaded that an error was committed in placing the planes obliquely to the impulse of the fluid, instead of diminishing the angle of incidence from 50 to 40 degrees; for it appears that when the planes formed the angle of 50 degrees with their path, the motive weight of 134 pounds produced a velocity of 4·575 feet in a second, and that the same weight gave exactly the same result when the planes, obliquity was reduced to 40 degrees. That this statement is incorrect is proved by a motive weight of 33½ pounds, which, when the angle was 50 degrees, drew the planes with the velocity of 2,283 feet per second, and at an angle of 40 degrees, 2,366 feet. In consequence, in the annexed Table I, I have rejected the experiment at the angle of 40 degrees, and combined those at the other angles, with some experiments not previously inserted in the *Annals.* The experiments were reduced to the same velocity, six feet in a second, by finding the exponent m of the resistance: according to this formula, $m = \frac{\log. R - \log. r}{\log. V - \log. v}$, R and r representing the motive weights 134 and 33½, and V and v the corresponding velocities. By calculating the exponent m for every angle of obliquity from 90 degrees to 10, nine values are obtained, the mean of the whole is 2·0154, or somewhat greater than the square of the velocity. The exponent m being found, the various resistances the planes met with at the different angles of incidence, and moving with different velocities, are reduced to the same velocity six feet per second by making $R = r \times \frac{V\,m}{v\,m}$, and the motive weights calculated by this formula are written down in Table I. To compare the experiments with more facility, the resistance of the two planes are reduced to the area of a square foot, or 42,885 pounds, and beneath these reduced resistances are placed the sines to radius 42,885 pounds, by which it appears that the sines exceed the resistances from 90 to 30 degrees. At this angle the numbers are nearly equal, the resistance being rather greater; afterwards the sines again exceed the resistances. This comparison might have been more readily made by dividing all the resistances by the greatest resistance, and under these ratios placing the natural sines which are to be found in many books of logarithms. These numbers and their squares are also inserted, and in no one instance do the experimented resistances coincide with the square of the sine. The reduced direct resistance when compared with the plane's resistance at six feet in a second, see Table II. is in excess 2½ pounds. This discrepance may be partly attributed to the larger surface of the two planes, and partly to subsequent improvements of the apparatus with which the experiments of Table II. were made. A motive weight of 42,885 pounds, according to the experiments in that table, would produce a velocity of 6,587 feet per second.

Two circumstances affecting the oblique resistance are the negative pressure and the accumulation of the fluid on that part of the plane on which it first strikes; that such an accumulation takes place is well known to practical men from the greater stress of the weather brace above the lee one of square sails hoisted by the middle, and forming an acute angle with the wind's direction. This fact is exemplified in the custom of slinging the yards of luggers by the thirds, by which one-third of the yard is before, and two-thirds behind the mast, and an equilibrium of the pressure of the wind is produced. That non-elastic fluids produce a similar effect was clearly shown in the rudder of the vessel built under the inspection of the late Earl Stanhope. His lordship, with the view of lessening, or rather annihilating, the labour of the helmsman, caused the pivots of the rudder to be inserted in the middle of the upper and lower surface, but on trial to produce the desired equipoise, it became necessary to shift the axis a third of the rudder's length, reckoning from the foremost extremity. From these observations, it appears that the oblique resistance of a plane consists of four parts, the impulse, the friction, the minus pressure, and an accumulation of the fluid on the fore part of the inclined surface. That these experiments are superior to any yet published may be inferred, without arrogating any great merit to those who made them, from the size of the experimented bodies, and to the accuracy of the apparatus which measured the velocity of the moving bodies to a small fraction of a second, a circumstance essentially necessary to obtain accurate results. It is most desirable that experiments of this kind should be repeated; and I anticipate with much pleasure the publishing of the second volume of Hydraulic Experiments made at Fahie Mines, in Sweden, under the scientific and able management of Messrs. Lagerhjelm and Kallstenius, at the expence of the Mineral Society.

In the *Annals* for October, 1815, Prof. Thomson did me the favour to insert a table of experiments on the direct resistance of water to a plane one foot square, and immersed to the mean depth of six feet, but owing to an inadvertency in copying the eighth column, entitled "Exponents of the Minus Pressure," was erroneous, and, therefore, expunged. I have now the pleasure of sending another, Table II, which does not materially differ from the former, excepting in the last column. The first contained the resistance of a square plane; the present, the mean resistance of a square and round plane, each containing 144 square inches, or one superficial foot.

Column 1 contains the velocity of the planes through the water in feet per second.

Column 2, columns of water, the base of each being one foot square, and the respective altitudes equal to the space through which a heavy body must fall to acquire the velocity of one, two, or three feet, &c. in a second.

Column 3, the weights of the different columns of water in lbs. avoirdupoise.

Column 4, the mean resistance of the two planes in lbs. avoirdupoise.

Column 5, the difference between columns 3 and 4.

Column 6, the minus pressure found by experiment.

In Column 7 is set down the exponents of the minus pressure. These exponents are found by calculating the various values of m answering to one and two feet, and the corresponding weights ·1616 and ·6075; then two and three feet, and the corresponding weights ·6075 and 1·2973, and so on as far as 12 feet. The mean value of the eleven exponents, 1,7646, is then used, and the table extended to 20 feet; but the same reliance is not to be placed upon the resistance of velocities exceeding 12 feet per second.

The minus pressure is thus determined: Figures 1 and 2 have the same fore and middle parts; the bows or foremost extremities wedges each oblique side, measuring three feet, and the base one foot; the middle part a cube one foot square; the stern or hinder part of fig. 1 is likewise a wedge whose oblique sides exceed the foremost by one foot and six inches, these sides being four feet and six inches long. It is evident by inspecting the two figures, that as their fore and middle parts are similar, the difference of resistance, after deducting the friction of the water (which in all cases, the planes excepted, has been done), must proceed from the form of the sterns. The experiments made with these bodies, 1 and 2, are contained in Table 3. To corroborate the above experiments, solids 3 and 4 were employed; these, like the former, had the same fore and middle parts, the bows circular, and the centres cubical, but in fig. 4, the wedge stern end was taken away; therefore the variation in the resistance in this case, as in the former, proceeds from altering the after extremity. The experiments made with these bodies, 3 and 4, are contained in Table 4, and show, like the former, a considerable increase of resistance. The minus pressure found by these last experiments is set down in Table 2. The minus pressure thus ascertained rests on the supposition that a wedge whose oblique sides exceeds the width of the base four times and a half is devoid of that kind of resistance. To clear up any doubts on this subject, experiments were made with bodies 5, 6, 7, 8, 9, 10; the stern ends of solids 6, 8, 10, were shorter than those of 5, 7, 9; the oblique sides of these measured in length three feet, and the oblique sides of the others four feet and one half. Tables 5, 6, and 7, contain the experiments, with these figures, and justify the conclusion that the minus pressure of those solids that have the longest after extremity is so minute that it may be considered as nothing, and consequently rejected.

By consulting Tables 3 and 4, it might be concluded that of all the variety of forms of which the stern end is susceptible, the most obtuse would have the greatest minus pressure. To prove or disprove the justness of this inference, the resistance of solids 11, 12, 13, were found, the last being the same as figure 2, that is, with a square stern. Fig. 12 is the same as fig. 8, turned end for end, or the hind part made the bow and *vice versâ;* but the stern of fig. 11 is an equilateral triangle. The result of these experiments is set down in Table 10, and it is very singular that an equilateral triangle so far from diminishing the minus pressure, augments it; and, on the contrary, a semicircular after body diminishes the minus pressure. The effect of joining an equilateral triangle to the base of an isosocles considerably augmented the resistance near the surface, as will appear from the following experiments: A wedge 43 feet in length, 4·75 feet in width, and 1,28 feet in depth, and nearly immersed, required a motive weight of 395$\frac{1}{4}$ pounds to draw it 12 feet in a second through the water by the vertex. On the addition of an equilateral triangle to produce the same velocity, 470 pounds were requisite, being an increase of 74$\frac{1}{2}$ pounds. That the shape as well as length contributed to diminish the negative pressure appears by comparing the result of fig. 12 with fig. 11, as contained in Table 8. The minus pressure of fig. 9 being nothing, it might be expected that the plus pressure or head resistance of this figure would be the same as the weight of water contained in Column 3 of Table 2; but on examining Table 2, the head resistance is smaller; consequently the plus pressure increases in a less ratio than the squares of the velocity.

Some observations on the size of the bodies, and the difficulties experienced in making the experiments, may not prove unacceptable to those who hereafter engage in a similar employment. It is recommended that the size of the bodies whose resistance is to be determined, should, if square, not exceed one foot in diameter; at first sight it might appear that the larger the surface, the more accurate the experiment, which, though true in theory, is false in practice, for large solids both by bulk and weight became unwieldy, and consequently difficult to manage; and when experiments were to be made beneath the surface of the water, considerable trouble occurred in accurately placing, and firmly securing, the immersed body to the conductor. When practicable, only one bar should be used in attaching the upper and lower bodies to each other; the shape of the bar should be elliptical with the transverse or longer diameter parallel to the centre line of the experimented body. This form of bar is advantageous on many accounts; it meets with less resistance, is not so liable to bend from the impulse of the water, and answers the purpose of a rudder by a small alteration in its parallelism, which, without affecting the accuracy of the experiment, will prevent the conductor from deviating with its attached solid from the intended course through the water. It would be a further improvement if the tremulous motion of the iron bar caused by its elasticity was prevented, which might be done by placing the bar in a metal case, and filling up the vacuity with melted lead. The part of the bar which passes through the conductor should be circular, that the figure attached may be placed accurately by turning the bar; and a mark should be made on the bar, and another on the upper part of the conductor, which ought to correspond when both are truly adjusted. At the bottom of the bar, a concave screw is cut, which fits into one of a contrary description that projects from the solid whose resistance is to be found. To prevent the bar from sinking, and being lost when necessary to detach it, the upper part was formed into a hook, and to this was fastened a line.

One of the principal reasons for recommending one bar in preference to two, originated from the loss of time, trouble, and vexation, in the year 1796, proceeding from the use of two. Many of the experiments made at that time were so extremely discordant as to induce a belief that the particles of water when once displaced did not arrange themselves in the same manner; but on reflection it was thought that the bars which were cylindrical, one inch and a half in diameter, and six feet asunder, were sufficiently close for the eddy water of the foremost to affect the resistance of the hindermost, which, on a further separation of the bars to nine feet, was found to be the case; at the same time it was deemed adviseable to alter the shape from a circular to an angular figure. In making experiments of this kind, it is necessary to have a considerable length of line to draw the figures by. To avoid the inconvenience of a high mast which, if a single pulley was used, would be unavoidable. A system of pulleys was adopted, and it was found from experience that two double blocks answered the purpose exceedingly well. The sheaves or wheels of the block were not placed in one shell, or side by side, but under each other; and the lower sheave of the block fastened to the top of the mast was smaller, and the uppermost sheave of the lower or movable block was also the smaller. By this arrangement every part of the line was parallel, and the uncertainty caused by the friction of the line against the sides of the frame avoided. The diameter of each of the larger wheels was ten inches, and that of each of the smaller, seven inches; and for the sake of lightness and appearance, they were inserted in iron frames, put together with nuts and screws, for the convenience of taking to pieces. A double cylinder would answer the purpose of a system of pulleys; but so much inaccuracy is caused by the friction of the line in winding as to exclude this contrivance.

The shape of the conductor represented by figure 14 is preferable to any other. The total length 26 feet, each oblique end six feet, the depth one foot, and the breadth one foot nine inches to two feet; the middle part was excavated within an inch of the bottom, sufficiently capacious to admit a quantity of iron or lead ballast to sink it and its attached body within an inch or somewhat more of the surface of the water. To prevent any alteration in the trim, the water should have access to the hollow by means of small holes, in which the ballast is stowed. The attached body or solid, whose resistance is the object of the experiment, ought to be rendered heavier than water by inserting cylinders of lead, so placed that the centre of gravity may be at the place where the bar is inserted.

Prior to the commencement of each day's experiment, the conductor, with the attached solid, should be weighed; that is, as much additional known weight placed on the conductor as will sink it level with the surface of the water. If, on a second day's trial less weight from the absorption of water is requisite to sink it, ballast must be taken out. The conductor is perforated in the middle, and near the commencement of the oblique stern, to admit the bar. A minute alteration when in motion of the horizontal position of the conductor, from the resistance of the water to the under body is made evident, by the water running above the thin edge of the bow or forepart. The edges of the bow and stern should be protected from injury by thin pieces of iron. The correct velocity, or the true resistance, is the latter part of the course; and after the line which gives motion is (if immersed during the run) above the water. Attention must be paid to this circumstance, or no accurate result can be expected; for the error will be in the compound ratio of the length and velocity of the line which is in contact with the water.

I remain, dear Sir, truly yours,

MARK BEAUFOY.

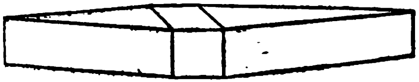
TABLE I.—*Resistance of a Plane exposed at various Angles to the Impulse of Water.*

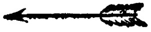
	Angles of Incidence.								
	90	80	70	60	50	40	30	20	10
					Velocity in Feet and decimal Parts per Second.				
Motive weight 134 lbs......	3·9562	4·0625	4·1812	4·3082	4·5125	4·8222	5·1000	6·4750	8·0750
Motive weight 33½ lbs......	1·9937	2·0156	2·0583	2·1666	2·2833	2·3666	2·5583	3·2915	4·2042
					Feet per Second.				
	6	6	6	6	6	6	6	6	6
Total resistance in lbs.....	310·19	294·06	277·48	261·21	237·94	208·15	185·39	114·92	73·644
Resist. of conductor subst...	55·30	55·30	55·30	55.30	55·30	55·30	55·30	55·30	55·30
Remains plane's resistance.	254·89	238·76	222·18	205·91	182·64	152·85	130·09	59·62	18·344
Reduced to one super foot.	42·855	40·171	37·382	34·644	30·729	25·717	21·887	10·031	3·086
Sines to radius 42355......	42·855	42·712	40·298	37·139	32·852	27·566	21·422	14·667	7·447
Ratio of resistances........	1·0	0·9367	0·8718	0·3459	0·7166	0·5997	0·5104	0·2339	0·0720
Sines of the angles.	1·0	0·9848	0·9397	0·8660	0·7660	0·6488	0·5000	0·3420	0·1736
Square of the sines.	1·0	0·9698	0·8830	0·7500	0·5868	0·4132	0·2500	0·1170	0·0301

Table II.—*Resistance of a Plane immersed to the Mean Depth of Six Feet.*

Feet.	Feet.	lbs.	lbs.	lbs.	lbs.		Feet.	Feet.	lbs.	lbs.	lbs.	lbs.	
1	0·0156	0·9750	1·1843	0·2128	0·1616	Expon.	11	1·8806	117·82	131·36	13·54	12·930	Expon.
2	0·0621	3·3860	4·6785	1·2925	0·6075	1·9015	12	2·2383	139·90	155·55	15·65	14·920	1·6675
3	0·1399	8·7450	10·395	1·6500	1·2973	1·8771	13	2·6270	164·18	181·89	17·71	17·184	1·6452
4	0·2487	15·543	18·278	2·735	2·2010	1·8376	14	3·0476	190·42	210·22	19·80	19·584	1·7646
5	0·3886	24·287	28·283	3·996	3·297	1·8110	15	3·4974	218·59	240·56	21·99	22·120	1·7646
6	0·5596	34·975	40·382	5·407	4·565	1·7848	16	3 9793	248·71	272·89	24·18	24·789	1·7646
7	0·7616	47·603	54·545	6·942	5·989	1·7613	17	4·4923	280·77	307·22	26·45	27·587	1·7646
8	0·9948	62·175	70·745	8·570	7·551	1·7356	18	5·0363	314·77	343·50	28·73	30·514	1·7646
9	1·2590	78·690	88·960	10·270	9·238	1·7120	19	5·6114	350·71	381·78	31·07	33·568	1·7646
10	1·5544	97·150	109·17	12·020	11·030	1·6827	20	6·2117	388·61	422·03	33·42	36·749	1·7646
1	2	3	4	5	6	7	1	2	3	4	5	6	7

Figure 1.





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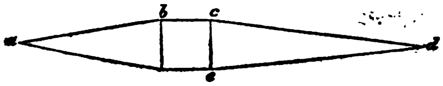
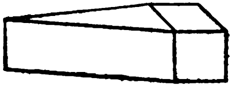


Ft. In.
a b = 3 0
b c = 1 0
c e = 1 0
c d = 4 6

Figure 2.



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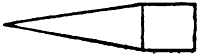


Table III.

	Velocity in feet per second.											
	1	2	3	4	5	6	7	8	9	10	11	12
Resistance of body 1, in lbs.	0·2512	0·9982	2·2001	3·817	5·813	8·153	10·811	13·756	16·959	20·400	24·060	27·890
Resistance of body 2, in lbs.	0·3730	1·4902	3·3104	5·793	8·899	12·594	16·843	21·619	26·891	32·640	38·830	45·424
Differences or minus pressure	0·1218	0·4920	1·1103	1·976	3·086	4·441	6·032	7·863	9·932	12·240	14·770	17·534

Ft. In.
fg = 1 0
gi = 1 0
gh = 4 6

Horizontal Section

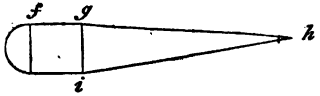
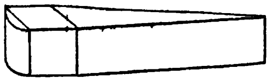


Figure 3.



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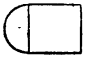


Figure 4.

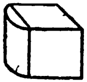


TABLE IV.

	Velocity in feet per second.											
	1	2	3	4	5	6	7	8	9	10	11	12
Resistance of body 3, in lbs.............	0·2362	0·9766	2·2148	3·935	6·118	8·747	11·805	15·275	19·140	23·397	28·010	32·980
Resistance of body 4, in lbs.............	0·3978	1·5841	3·5121	6·136	9·415	15·312	17·794	22·826	28·378	34·43	40·940	47·900
Differences or minus pressure	0·1616	0·6075	1·2973	2·201	3·297	4·565	5·989	7·551	9·238	11·03	12·93	14·92

Figure 5.

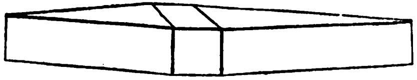


Figure 6.

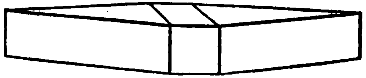


TABLE V.

	Velocity in feet per second.											
	1	2	3	4	5	6	7	8	9	10	11	12
Resistance of body 5, in lbs.	0·2512	0·9982	2·2001	3·817	5·813	8·153	10·811	13·756	16·959	20·400	24·060	27·89
Resistance of body 6, in lbs.	0·2149	0·8820	1·9887	3·514	5·437	7·741	10·402	13·406	16·734	20·371	24·300	28·50
Differences	0·0363	0·1162	0·2114	0·303	0·376	0·412	0·409	0·350	0·725	0·029	0·240	0·61

Figure 7.

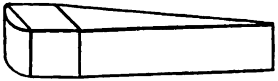


Figure 8.

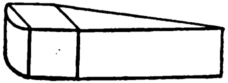


TABLE VI.

	Velocity in feet per second.											
	1	2	3	4	5	6	7	8	9	10	11	12
Resistance of body 7, in lbs.	0·2362	0·9766	2·2148	3·935	6·118	8·747	11·805	15·275	19·140	23·397	28·010	32·98
Resistance of body 8, in lbs.	0·2410	0·9962	2·2599	4·017	6·247	8·939	12·071	15·625	19·590	23·953	28·700	33·80
Differences.	0·0048	0·0196	0·1451	0·082	0·129	0·192	0·266	0·350	0·450	0·556	0·690	0·82

Figure 9.

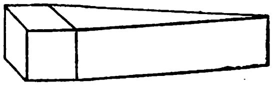


Figure 10.

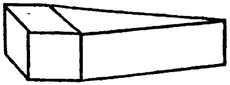


TABLE VII.

	Velocity in feet per second.											
	1	2	3	4	5	6	7	8	9	10	11	12
Resistance of body 9, in lbs.	0·9061	3·6211	8·089	14·252	22·057	31·466	42·429	54·908	68·870	84·280	101·10	119·29
Resistance of body 10, in lbs.	0·9322	3·7081	8·258	14·517	22·430	31·945	43·020	55·610	69·690	85·160	102·07	120·48
Differences	0·0261	0·0870	0·169	0·265	0·373	0·479	0·591	0·702	0·820	0·880	0·97	1·19

Figure 11.

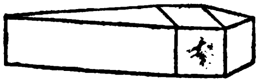


Figure 12.

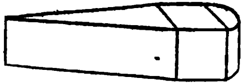


Figure 13.

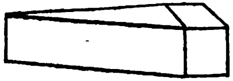


TABLE VIII.

	Velocity in feet per second.											
	1	2	3	4	5	6	7	8	9	10	11	12
Resistance of body 11, in lbs.	0·4526	1·7924	3·9619	6·908	10·582	14·944	19·954	25·574	31·770	38·510	45·780	53·530
Resistance of body 12, in lbs.	0·2766	1·1437	2·5967	4·620	7·194	10·504	13·930	18·059	22·670	27·760	33·300	39·270
Resistance of body 13, in lbs.	0·3730	1·4902	3·3104	5·7930	8·899	12·594	16·843	21·619	26·891	32·640	38·830	45·424

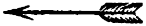
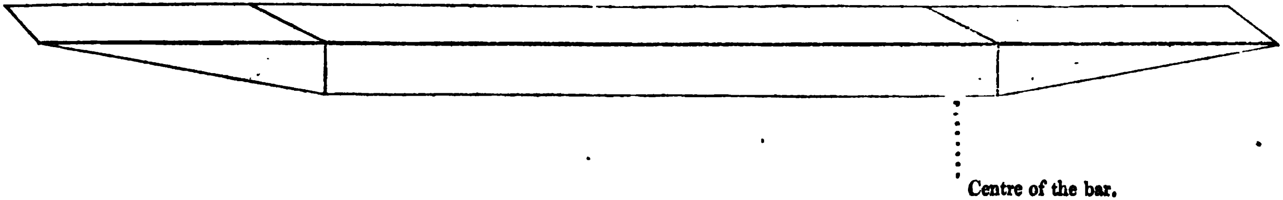


Figure 14.



ARTICLE IV.

Tabular View of a Meteorological Journal kept at Lancaster. By Mr. John Heaton.

1821.	THERMOMETER.						BAROMETER.					WEATHER.		WINDS.							
Months.	Highest.		Lowest.		Mean.	Mean diurnal variation.	Highest.		Lowest.		Mean.	Wet days.	Fair days.	S.	SW.	W.	NW.	N.	NE.	E.	SE.
January .	19	50	4	24	39·1	6·2	23	30·80	9	29·03	29·87	11	20	10	5	3	0	0	3	9	1
February.	1	49	12	26	38·4	10·0	5	30·65	28	29·27	30·26	3	25	2	7	2	1	3	2	7	4
March...	29	57	23	31	43·0	10·8	15	30·32	28	29·01	29·56	16	15	9	11	3	3	1	1	1	2
April . . .	26	69	6	34	50·0	12·4	30	30·14	3	28·94	29·62	18	12	2	15	4	0	1	2	3	3
May	5	66	26	35	50·8	13·1	20	30·29	15	29·17	29·89	14	17	3	5	9	5	3	1	2	3
June....	29	72	27	43	56·6	14·8	18	30·51	7	29·69	30·16	3	27	0	2	6	1	3	10	6	2
July	19	75	2	43	60·2	11·0	18	30·35	22	29·45	29·94	12	19	1	8	13	3	1	3	0	2
August . .	22	77	19	50	61·3	11·3	20	30·23	10	29·27	29·93	14	17	2	12	5	1	0	0	7	4
Sept.....	4	72	30	48	59·0	8·2	15	30·21	29	29·19	29·80	18	12	8	11	7	1	0	1	2	0
October..	17	62	16	38	51·5	8·8	13	30·33	28	28·81	29·82	20	11	4	13	3	1	1	0	2	7
Nov.....	15	58	5	35	47·1	7·0	6	30·25	26	29·00	29·65	19	11	0	14	6	2	1	0	3	4
Dec.....	10	54	26	33	43·4	6·4	11	30·24	28	28·34	29·35	22	9	5	7	6	2	2	0	2	7
	Aug.	77	Jan.	24	50·0	10·0	Jan.	30·80	Dec.	28·34	29·82	170	195	46	110	67	20	16	23	44	39

Article V.

Reply to C's Observations on Mr. Herapath's Theory.

(To the Editor of the *Annals of Philosophy*.)

Sir,

If you think the following answer to C.'s observations on Mr. Herapath's theory worthy of a place in the *Annals of Philosophy*, it is at your disposal. I am Sir,

Your obedient servant, D.

"He who has published a theory on any point in philosophy," your correspondent C. gives us to understand in his attack on Mr. Herapath, "has no right to complain" of any observations tending to expose its fallacy; and from the mode of his own attack we may, I suppose, add, in whatever manner they may be made. Without inquiring into the soundness or propriety of this new doctrine, we may with justice affirm, that when a man sets himself up as a judge in scientific matters without being fully competent, and acrimoniously attacks the theory of another without well understanding it, or the subject, "his exposure is all he ought to expect." Should it appear, as I think it will, that C. has done both of these things, "he will, therefore, have no right to complain of the following observations" in reply to his.

C. sets out with an observation well calculated to give us a high opinion of his inductive accuracy. Mr. H. had said, "It is impossible by correct reasoning from false principles to bring out true conclusions." The axiomatic evidence of this position no one, I think, can dispute, if what is evidently implied be considered; namely, that the reasoning as well as correct must be complete by including all the circumstances which bear on the case. But C. says, "In *innumerable* instances *true* conclusions may be brought out by *correct* reasoning from false principles. If, for instance, the *errors* on each side should exactly compensate each other, the result will be correct, though the foundation be erroneous." So then *correct* reasoning must contain *errors*; that is, I apprehend, *truth* must be *error*. Of course, by parity of argument, *false* reasoning must contain *no* errors, or *error* must be *truth*, and *wrong, right*. Is it not a happy thing Newton did not know, or did not believe this? How is it after so "conclusive an argument," C. thought it necessary to continue his "observations?" Would not this "beautiful reasoning! and invincible demonstration!" at once crush the whole of Mr. H.'s theory? What does it matter about their having no connexion with the subject? C.'s reasoning has "the distinguished excellence" of disproving equally well not only the thing he would wish, but every thing else, whether connected with it or not.

Alluding to the loss and development of heat in the changes of state, C. objects to Mr. H.'s theory of heat by motion, "because heat may for a time become imperceptible, and again be developed, without being destroyed? "If, therefore," says C. "heat and motion be identical, motion cannot be destroyed, which the experience of every day tells us is untrue." Here C. would plainly charge Mr. H.'s theory as being incompetent to explain, nay, as being repugnant to the phænomena of latent heat. Now observe, Mr. H.'s "Theory of the Changes of State and the Concomitant Phænomena," in which the subject C. alludes to is copiously explained, was published in the *Annals* for October; C. in his "Observations," dated nearly a fortnight afterwards, tells us he had seen this very number of the *Annals*, and of course this very explanation, for the want of which he gravely tells the world Mr. H.'s theory is defective. Perhaps C.'s creative talent can give some acceptable form to this nondescript offspring of his fertile invention? If this cannot be done, C. will find in the December number the mathematical laws of the defect he complains of numerically confirmed by the experiments of Ure, Thomson, Dalton, Southern, Watt, Black, &c. Probably the experimental testimonies of these philosophers may induce a conviction of the validity of Mr. H.'s views, which, it is to be hoped, C. will have liberality enough to acknowledge.

Speaking of the gravific medium which Mr. H. confesses to have adopted from Newton, C. says: "*Show* me this fluid; *prove* its existence." In the name of common-sense, and of all that is reasonable, who, besides C. could have made so unaccountable a request? What reply could C. expect from Mr. H. to such a demand but this very natural one? "Show me your one or two fluids of electricity, of galvanism, and of magnetism; show me your favourite fluid of caloric; show me these, or either of them, and by the very same means I will show you the fluid you desire. "Prove the existence" of attraction, and by that identical method, or those identical phænomena, I will 'prove the existence' of my gravific fluid. "Besides," Mr. H. might add, "I will do more; I will 'prove its existence,' as I have in p. 411 to 415, *Annals* for June, by other phænomena to whose solution you cannot apply the vulgar notions of attraction." This would be the natural reply of Mr. H. or of any one, to so unexampled a demand. But the oddity of this odd request is, "*Show* me this fluid." Surely C. does not require Mr. H. to make this fluid visible? He does not wish, does he, Mr. H. to catch and bring to him a nameless being, a few particles of a fluid, which Newton says is so extremely subtile as to be able to pervade the pores of the densest bodies with the utmost facility? If such be C.'s desire, I feel persuaded Mr. H. will readily undertake the task, if it be only to satisfy his incredulity, provided C. will show Mr. H. how to succeed.

Accuracy, it seems to me, should be rigidly adhered to in all discussions. An author should never be made appear to say what he has not. In more than one instance, C. has, I think, not been over delicate in this respect. At present, I shall adduce an example which will serve as a specimen of the rest; and lest there should be any mistake or difficulty in turning to Mr. H.'s opinion, I shall place right against it one or two quotations from his first paper.

Quotations from

C.'s "Observations on Mr. Herapath's Theory," *Annals* for Dec. 1821, p. 420.	Mr. Herapath's paper, *Annals* for April, 1821, p. 279.
	"Therefore it appeared to me that the ultimate atoms ought to possess two properties in *direct contrariety*, hardness and elasticity."*
"But whether the atoms be elastic or hard, *having the properties of elastic bodies which Mr. H. has attributed to them.*"	*Schol.* Prop. II. p. 285 and 286. "Hardness and softness are diametrically opposite properties, and *elasticity* is nothing but an *active kind of softness.*" "To argue*** is to abandon the definition of *hardness*, and to adopt that of *elasticity*, which has *no connexion* whatever with it." See also Mr. H.'s Definitions, p. 282.

These quotations exhibit too marked a contrast for comment to increase. It will exercise C.'s ingenuity to identify them; but it is to be hoped C. has not taken advantage of an anonymous signature to say what would press too heavily on the credit of a name.

C. speaks of Sir Isaac Newton, and insinuates to the world that Mr. H. is trying to overturn him. Except in the absolute equality of reciprocal attraction in the planets, which Newton deduced merely from analogy, and of which no proof whatever can be furnished, there is no one phænomenon in which Mr. Herapath does not perfectly agree with Newton. Indeed Mr. H. is almost the only philosopher of the present day who has not arrogated to himself the liberty, on the most trivial grounds, of opposing that great philosopher; and it must be no little gratification to Mr. H. that while his discoveries fully confirm the views of that illustrious man, they have so stable and indestructible an authority as that of Bacon and Newton. But since C. opposes Newton to Mr. H. I beg to ask him on what grounds he does it? Is it on the doctrine of heat? If it be, he must excuse me for publicly telling him that Newton's and Mr. H.'s views of the nature of heat coincide; they both conceive heat to consist in motion. Perhaps C. who takes great pains to appear to know something of Newton's works, is not aware of this. That he is, by his observations, unacquainted with it, though one of the commonest of Newton's ideas, is evident; for we can hardly bring ourselves to believe, if he knew it, that a man so peaceably inclined as to commence a violent controversy without cause or provocation, and moreover so modest as to withhold his name from an attack as virulent as it is violent, could quarrel with Mr. H. for following one, whom he, C. does indeed really profess to admire.

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* Mr. H. has written softness, but immediately before he tells us that "elasticity is nothing but active softness;" and he now, therefore, uses softness instead of elasticity merely to make the contrast the stronger.

Let us, however, examine C.'s objections to the theory of heat by motion. He says if two bodies be placed in contact, the one having larger particles than the other, that the temperature of the body with the larger particles, though at first equal to the other, will continually increase from the mere contact and unequal size of the particles. For, says C. " it is evident that the atoms of A" (the body having the smaller particles) " may impinge upon the atoms of B, whether they be approaching A or receding from it; that is, the atoms of A having a greater velocity may either meet or overtake the atoms of B; and the probabilities will be nearly equal the one or the other." This I grant is nearly correct; but C. goes on; " if one atom *a*, of the body A, having a greater velocity than the atom *b*, of the body B, overtake the slower atom, the atom *a* will lose some of its velocity which will be communicated to the atom *b*, and thence among the other atoms of the body B. The communication of motion from the atoms of A to the atoms of B will not be compensated; for the atoms of B having less velocity than the atoms of A, *will never* overtake them." Hence by this " Beautiful reasoning! Conclusive argument! Invincible demonstration! as self-evident as that two and two make five" (C.'s own words), he concludes, that " the temperature of the body B shall *continually increase*." What becomes of the temperature of A, I do not know; C. has not told us; but I suppose as " the temperature of B shall *continually* increase," that of A increases too. Hence we have another source of heat we did not know of before. It is only to put two bodies in contact with unequal particles, and we shall have heat generated without the aid of friction or percussion; and without chemical, galvanic, or electric action. And all this results, by C.'s mathematics, from a theory fathered by Bacon, and supported by Newton; namely, that heat consists in motion. What a wonderful discovery! "a discovery," to use the words Sir H. Davy has employed on another occasion, "that seems to have been reserved for C. and the year 1821;" a discovery, it is plain, that makes it "as self-evident as that two and two make five;" that Bacon and Newton, as well as Mr. Herapath, "have in truth *quite* mistaken the road to philosophical science."

Having proved the importance of C.'s discovery, let us consider a little more attentively whether it be really a consequence of Mr. Herapath's theory, or of C.'s "invincible" mathematics. Mr. H.'s Prop. 4, in the *Annals* for April, 1821, stands thus: "If a hard body overtake and strike another hard body, moving with a less velocity in the same right line, the first body will, after the stroke, continue its course with the same velocity which the other body had before it; and the second body will acquire from the stroke a momentum equal to the difference of the velocities of the bodies previous to the contact, drawn into the mass of the first body; that is, if A B represent the two bodies, and a b their velocities before collision, the motion of A afterwards will be A b, and that of B, B b + (a − b) A." Hence conceiving that the particles of each body move uniformly and respectively with their mean velocities, which is the precise case C. has considered, it follows in the case of A overtaking B, that B will return to its body with the motion 2 B b − b A; and A, instead of returning to its proper body, will continue to move towards the other body with the motion A b, until it meet with B, or some other particle, in its exit from the body. For A cannot now overtake another particle, because its velocity from the last collision is reduced to the same as that of the particles of the other body; nor can it return to its own body, because the collision did not give it an inward, but merely diminished its outward, motion. Now the outward particle which A next strikes must evidently meet it with the mean motion B b of the particles to which it belongs. By Mr. H.'s Prop. 5, of his first paper, an exchange of motion between A and the second struck particle will take place; A will return to its body with the motion B b or A a, and the particle struck to its body with the motion A b. The motion, therefore, which is communicated to the body to which A belongs by the return of this particle, under the view in which C. would consider it, is A a; that is, the same as the proper motion of the particles of the body; and the total motion with which the two particles struck return to their body is 2 B b − A b + A b = 2 B b; that is, precisely the same as the sum of the mean motions of any two of its particles. Consequently the temperature of the body, which C. says ought to be augmented, is neither augmented nor diminished, by being in contact with a body of an equal temperature having particles less in size.

The circumstance under which I have considered this, corresponds to the mean circumstances of the case. I have omitted to consider the unequal motions of the particles arising from their mutual attraction, which will sometimes make them strike with a less, and sometimes with a greater, than their mean force; but which ultimately come to the same thing, as if they were all mutually moving among one another within prescribed paths, with a velocity uniform for the particles of each body. However, though this omission will make no difference on the mean communication of motion from one body to the other, it will, however, make a considerable difference in one case on which C. has ventured to deliver his opinion. C. says, "the greater atoms having less velocity than the less will *never* overtake them." This is not universally the case. In consequence of the mutual action of the particles, they move both in their goings and returnings swifter at some parts of their paths than at others. Generally speaking, in the exterior particles, which are those of the two bodies that come in contact, their velocities are the swiftest immediately before and after the collision; and the slowest immediately preceding and following the exterior extremity of their path. Hence, therefore, the greater particles may often move much swifter than the less; and consequently may frequently overtake and strike them, notwithstanding C asserts the contrary. This little circumstance will, perhaps, help to show C. that his haste in this attack on Mr. H. exceeds his judgment, and his temerity his depth.

From the views I have just taken, it follows that if two bodies be brought into contact, having unequal temperatures, and nothing foreign interferes, they will ultimately have the same temperature; the particles of the body with the higher temperature communicating just so much of their excess of motion, as will give to the particles of the other body, individually, a momentum equal to their own reduced momentum. For as the particles strike one another in all directions, the differences of temperature which are momentarily communicated to each body by the contact, are distributed as soon, or almost as soon, as communicated, by the successive particles in every direction. By this means, the motion of the particles which first received the difference of temperature becomes presently affected in the very opposite direction to that in which the difference was first communicated; and consequently the difference between the communicated motions from body to body becomes less. And thus this difference continually diminishes until the two bodies attain a common temperature. This very simple and obvious consequence I should not have taken the trouble to explain, had not C. drawn conclusions on this subject too absurd to be entertained by any other person, I apprehend, but himself. Because by Mr. H.'s theory of collision, when two perfectly hard bodies meet moving in opposite directions, all exchange of momenta takes place, C. concluded that if an absolutely cold body were brought to touch a warm one, no matter how great its temperature, the hot body would become absolutely cold, and the cold one would become as hot as the other was; and this, I believe, is to hold good whether the bodies have an equal or an unequal number of particles. Hence if the cold body should contain a greater number of particles than the hot one, motion must, to an indefinite extent, be generated by the mere contact; and if the cold body contain a less number, motion must by the same means be indefinitely destroyed. It is impossible to tell when one considers these ridiculous conclusions what we ought rather to do—to smile at the folly, or to pity the absurdity and presumption of the man, who could thus venture to utter to the world such things as the legitimate consequences of a theory, supported by Bacon, Des Cartes, and Newton!!

I have now shown, so far as C. has objected to it, that the theory of heat by motion is not incompatible, but perfectly compatible, with phænomena. He that desires to see the theory amply and fully expounded, may consult Mr. Herapath's last paper in the *Annals*, from July, 1821, to January, 1822. It will there be found that Mr. H. has not clothed his theory in the deceitful garb of general reasoning, but has reduced it to mathematical and numerical laws; and has defended the whole by a phalanx of facts, which it would, perhaps, put even the confidence of C. to the blush to oppose.

(*To be concluded in our next.*)

ARTICLE VI.

On the Crystalline Form of Yellow Copper Ore. By William Phillips, FLS. &c. *With an Analysis.* By Richard Phillips, FRS. L. and E. &c.

YELLOW copper ore occurs in Cornwall in different states; namely, crystallized, amorphous, and mamillated, the latter variety sometimes passing into botryoidal and stalactitic.

Every mineralogist, beginning with Romé de Lisle, has to the present time considered the ordinary crystalline form of the yellow copper ore to be the regular tetrahedron, which also has been assumed to be the primary form of its crystals, except by Mohs, who considers it to be an octohedron, with a square base, and who notices cleavages parallel to its planes.

I have for several years been in possession of regular cleavages of this substance with perfectly brilliant planes, and even of the primary octohedron produced by cleavage, without, however, having been able to satisfy myself as to the manner in which that octohedron lies (if I may so express myself) in the tetrahedron, which is the prevailing form of the crystals; and I should have found it extremely difficult to satisfy myself on this head, without the assistance of M. Levy, whose mathematical and crystallographical acquirements are too well known to need a comment by me.

The tetrahedron in which pyritous copper most commonly occurs, but which is never to be found, as far as my observation extends, without what may be termed the replacement of its solid angles, is so nearly allied to the regular tetrahedron, that it is not surprising it should have deceived the eye of the mineralogist, even when assisted by the application of the common goniometer to its planes, since the two tetrahedrons differ but very little from each other in measurement.

Fig. 1.

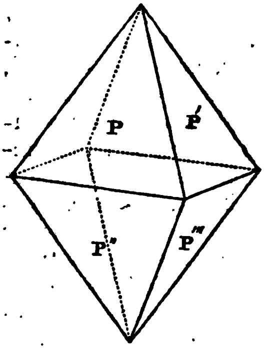

Fig. 2.

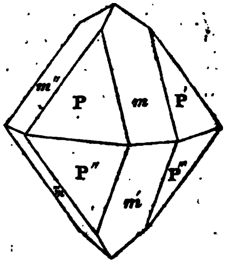

Fig. 3.

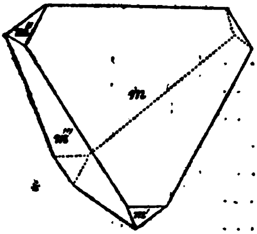

Fig. 4.

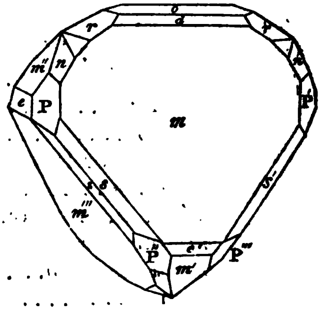

Fig. 1 represents the primary octohedron, which is more acute than the regular octohedron, the measurement of P on P′ or P″ on P″ being 101° 52′, and that of P on P″, or P′ on P‴ being 126° 30′: these measurements were taken on brilliant planes of cleavage by the reflective goniometer.

Fig. 2 represents the primary octohedron having the edges of its pyramids, though not of their common base, replaced; and it so occurs in Derbyshire.

Fig. 3 represents the ordinary form of its crystals; namely, a tetrahedron having its angles replaced, and in the direction in which it will most obviously appear to be constituted of the planes *m m′*, *m″ m‴*, of fig. 2. Hence the tetrahedron is a secondary crystal, arising from the complete replacement of all the primary planes by those which truncate the edges of the primary crystal. It exhibits a remarkable deviation from that symmetry of form generally so apparent in substances of which the primary crystal is a perfectly regular solid; for here, although the crystal is so far symmetrical that the planes are alternately large and small, it deviates by so much from that more perfect equality in the proportions of the secondary planes observable in those belonging to regularly geometrical primary crystals.

Fig. 4 represents a crystal in my possession from Cornwall, in which all the planes, except two or three, are sufficiently brilliant for the use of the reflective goniometer, and this crystal, which exhibits planes that are not very common, or rather that are very uncommon, might alone be assumed as affording sufficient evidence that the primary form is not the regular tetrahedron. Annexed are the measurements; but it seems requisite to premise, that angles taken on natural planes, however brilliant, are rarely perfectly accurate, and in proof that the following are not so, it will be observed that P on P′ taken on the natural planes is 102° 15′, but on planes of cleavage 101° 52′, as before stated; but taking the latter as the basis of his calculation, these measurements will assist the mathematician; for experience leads me to the conclusion that the difference between truth and error lies generally within the narrow compass of 30′.

P on P′, or P″ on P‴	102	15
P on P″, or P′ on P‴	126	30
P on *r*	151	25
P on *n*	169	32
P on *m*, or P′ on *m*.	141	15
m″ on *m*	141	15
m′ on *m*, or *m″* on *m‴*	108	35
m″ on *e*, or *m′* on *e′*.	144	20
m on *m* over *o*.	71	35
o on *m*	126	30
l on *l*.	149	2
n on *r*	160	54
r on *r′*	133	50
r on *m*	143	24
n on *e*	144	10
n on *n′*	111	50

Analysis.

I cannot find that crystallized yellow copper ore has ever been subjected to analysis, but the amorphous has been analyzed both by Lampadius and Gueniveau. According to the former, it consists of

Sulphur	43·70
Iron	16·57
Copper	39·73
	100·00

According to Gueniveau, taking the mean of two analyses, one specimen being from Sainbel, and the other from Baigorry, this ore (omitting a small extraneous admixture) consists of

Sulphur	36·36
Iron	33·00
Copper	30·64
	100·00

To analyze this ore, I proceeded in the mode described in page 86 of the present volume. One hundred grains reduced to powder were heated in a mixture of nitric and muriatic acid until the whole of the sulphur was acidified. Half a grain of earthy matter was left undissolved; to the clear solution, nitrate of barytes was added in excess; and the sulphate of barytes obtained, after washing and drying, weighed 259·3 grains.

The excess of barytes being separated by sulphate of soda, the clear solution was supersaturated with ammonia so as to dissolve the oxide of copper, and precipitate the oxide of iron; the latter washed and dried, weighed 46 grains.

The ammoniacal solution of copper was heated with potash so as to evaporate the whole of the ammonia, and reduce the copper to the state of peroxide: this, washed and dried, weighed 37·5 grains.

According to Dr. Thomson, 118 of sulphate of barytes are equivalent to 16 of sulphur; 259·3, therefore, indicate 35·16: 40 of peroxide of iron contain 28 of metallic iron; 46 will give 32·2; and as peroxide of copper contains one-fifth of its weight of oxygen, 37·5 are equal to 30 of copper.

On adding together these products, it will be found that with the earthy matter they make 97·86, leaving a deficiency of 2·14 in the 100 parts of ore.

As this loss is so considerable, I repeated the analysis as far as regards the copper, in which the error was suspected to exist; but I obtained precisely the same quantity of peroxide as at first. I, therefore, examined the solution of potash with which the oxide of copper had been boiled. This solution was saturated with nitric acid, carbonate of soda was added to it, and a white precipitate was formed, which was blackened by sulphuretted hydrogen; it was, therefore, probably oxide of lead. Another portion of solution saturated with nitric acid gave a precipitate with nitrate of lead, after all the sulphuric acid had been thrown down by nitrate of barytes. It is evident, therefore, that some arsenic acid was present.

Crystallized yellow copper ore appears, therefore, to consist of

Sulphur	35·16
Iron	32·20
Copper	30·00
Earthy matter	0·50
	97·86
Lead, arsenic, and loss	2·14
	100·00

If we neglect the small quantity of lead, arsenic, and earthy matter, as extraneous, it will appear that my analysis of the crystallized ore agrees so nearly with Gueniveau's statement of the composition of the amorphous variety, that they may be considered as differing only in form.

The mamellated variety was next submitted to analysis: this is much less common than the other varieties, and I am not sure whether it occurs in any other place than Cornwall. It is thus described by Count Bournon, in the Philosophical Transactions for 1801, under the name of yellow hematitic copper ore: "This kind of copper ore is sometimes of a deep yellow colour, which inclines the more to green, as it is destitute of brilliancy. It is very compact, and, when broken, the fracture appears smooth, sometimes a little conchoidal; its surface, however, has a very fine grain, which, when viewed with a powerful lens, resembles the aggregation of a very close compact mass of the finest sand." It is afterwards stated that it occurs mamillated, botryoidal, and in the form of small cylinders; and by the decomposition of the surface, it acquires violet, blue, and green colours.

In the same volume of the Transactions, Mr. Chenevix has given an analysis of this ore, according to which it consists of

Sulphur	12
Copper	30
Oxide of iron	53
Silica	5
	100

The first observation which occurs with respect to this analysis is, that there does not exist, as far as I recollect, any mineral which consists of a sulphuretted metal in combination with an oxide. Added to this, it is to be observed that the sulphur exceeds by 4·5 the quantity required to form a protosulphuret with the copper, and is deficient 3 to form a persulphuret.

On examining Mr. Chenevix's process, it will be seen that he neglected to examine the nitric solution of the copper and iron for sulphuric acid, which must have been formed while the nitric acid was dissolving these metals, and the quantity of sulphur was determined merely by weighing the portion remaining unacidified. This circumstance will account for a part at least of the deficiency which Mr. Chenevix has attributed to oxygen; and further, to prove the iron exists in the state of oxide, he says, that the greater part of the iron, but none of the copper, is dissolved in muriatic acid. I must confess that I have obtained different results. After long boiling in muriatic acid, the ore lost only seven per cent. and of this a part was copper.

I found that this ore contained a little arsenic, but I did not discover traces of any other metal, excepting iron and copper. I performed the analysis in the mode already described, and obtained from 100 grains, 1·1 of insoluble earthy matter, 254·2 of sulphate of barytes, 44 of peroxide of iron, and 39 of peroxide of copper; and according to what has been already stated of the composition of these substances, the ore consists of

Sulphur	34·46
Iron	30·80
Copper	31·20
Earthy matter	1·10
Arsenic and loss	2·44
	100·00

Now these proportions differ, excepting in the quantity of copper, most materially from the results of Mr. Chenevix; but they agree so nearly with those obtained by Gueniveau from the amorphous and by myself from the crystallized variety, that I trust it will be evident that all the varieties are similarly constituted; and I shall now attempt to show their atomic constitution.

A compound of two atoms of protosulphuret of iron and one atom of persulphuret of copper, would consist of

4 atoms of sulphur 16 × 4. .... = 64
2 atoms of iron 28 × 2........... = 56
1 atom of copper................ = 64
184

And 100 parts will give

Sulphur	34·78
Iron	30·44
Copper	34·78
	100·00

With respect to the crystallized yellow copper ore, it will be seen that if we neglect the arsenic, lead, and earthy matter, as extraneous bodies, and supply the deficiency with copper, 100 parts will consist of

Sulphur	35·16
Iron	32·20
Copper	32·64
	100·00

Still leaving the copper about two per cent. too little, and the sulphur and iron in excess. If, however, we adopt the same plan with the analysis of the mamellated yellow copper, we shall have a very near approximation to the theoretical composition which I have suggested; viz.

Sulphur	34·46
Iron	30·80
Copper	34·74
	100·00

Article VII.

On the Influence of Humidity in modifying the Specific Gravity of Gases. By Thomas Thomson, MD. FRS. Regius Professor of Chemistry in the University of Glasgow.

A friend of mine, whose intelligence and candour I estimate very highly, mentioned to me some time ago that he considered the specific gravity of hydrogen gas given in my paper published in the *Annals of Philosophy*, vol. xvi. p. 168, as inaccurate; because the gas had not been previously freed from moisture; and being collected over water, must have contained as much vapour as was compatible with the temperature at which the specific gravity was taken. This objection renders it proper for me to enter somewhat more into detail than I did in that paper, in order to show how far my mode of experimenting guarded against this obvious source of inaccuracy. I do this the more willingly, because it will give me an opportunity of calling the attention of chemists to a property of vapour, ascertained indeed more than ten years ago; but which does not seem to have yet attracted the attention of scientific men; at least I am not aware of any allusion to it in any of the systematic works on heat, which have lately appeared.

In the second volume of the second series of the Manchester Memoirs, published in 1813, there is a paper by John Sharpe, Esq. entitled "An Account of some Experiments to ascertain whether the Force of Steam be in Proportion to the generating Heat." In this paper, Mr. Sharpe relates experiments proving the truth of the two following propositions: 1. Water heats equably, or in the same time (supposing the heating cause the same) from 120° up to the highest temperature which it can reach without boiling (and that temperature depends upon the pressure). Suppose, for example, that it is heated 10°, or from 120° to 130°, in three minutes, it will be heated from 270° to 280° in the same time. The reason of this equality I suppose to be that the quantity of heat constantly flowing into the water from the fire (or the difference between the temperature of the fire and water) is so considerable, that the 150° or 200° of heat which have been added have no sensible effect in diminishing that difference. 2. Six ounces of steam of 212° condensed into water give out as much heat as six ounces of steam of the temperature 275°; but the second six ounces come over in a much shorter period than the first.

M. Clement, whom I had the pleasure of seeing in Glasgow about two months ago, informed me that he had verified this last experiment of Mr. Sharpe at different temperatures; and what adds to the value of these determinations is, that he was not aware of Mr. Sharpe's experiments till I pointed them out to him in my own library. Thus the experiments of Mr. Sharpe and M. Clement serve mutually to confirm each other, and entitle us to draw the following conclusion from them: *Whatever be the temperature of steam from* 212° *upwards, if we take the same weight of it, and condense it by water, the temperature of the water will be always elevated the same number of degrees.*

It follows from this general law that the latent and sensible heats of steam (reckoning from 32°) added together always form a constant quantity, whatever be the temperature of the steam. This puts it in our power to determine the latent heat of steam at every other temperature, provided we be acquainted with it at the temperature of 212°. Now the latent heat of steam at 212° I believe to be 1016°. The sensible heat of steam at 212° (reckoning from 32°) is 180°; consequently the sensible and latent heats of steam at 212° added together make up the quantity 1196°. And this being the amount of the latent and sensible heats of steam at every temperature, the method of determining the latent heat of steam at all temperatures becomes self evident. The following table exhibits the sensible and latent heats of steam at a variety of different temperatures:

Temp. of the steam.	Sensible heat.	Latent heat.
32°	0°	1196°
50	18	1178
100	68	1128
150	118	1078
200	168	1028
212	180	1016
250	218	978
300	268	928
344	312	884
500	468	728

The inspection of this table, which the reader may easily extend *ad libitum*, will enable us to explain several phenomena, which, though they have been long known, have not yet, so far as I know, been satisfactorily accounted for. Mr. Watt, for example, found, that water could be distilled over in a vacuum very well at the temperature of 70°; but to his great astonishment, the latent heat of the vapour was just as much greater than the latent heat of steam at 212°, as the temperature of 70° was lower than 212°. Now, from the preceding law, it is obvious that this must be the case, and that more fuel is required to distil water in vacuo than in the open air. One of the advantages which Mr. Woulfe stated as belonging to his high pressure engines was, that they performed more work with a less expence of fuel than the ordinary steam engines. And I have been told by more than one Cornish gentleman conversant with these engines, that they really save a considerable quantity of fuel. Now it is easy to see from the preceding table taken in conjunction with the known increase of the elasticity of the steam at high temperatures, that this must be the case. The elasticity of steam at the temperature of 344° is eight times greater than at 212°; while, at the same time, the latent heat is 132° less. It is necessary indeed to raise the sensible heat of the water to be converted into steam 132° higher than 212°. But this is an expenditure of fuel only made once for all; for the water, when once heated to that temperature, may be kept at it with comparatively little fuel.

Thus steam is employed with the greater economy the higher the temperature to which it is raised. But the great strength necessary for vessels containing high pressure steam, and the greater liability of these vessels to be injured, necessarily sets a limit to the temperature to which the steam can be raised.

This law, to which the latent heat of vapour is subjected, has struck several persons to whom I have stated it with surprise; yet it is perfectly analogous to what takes place in other bodies. Thus it is well known that the specific heat of common air increases in proportion to its expansion. This is the reason why the temperature of the air diminishes in proportion as we ascend in the atmosphere. Now the latent heat of vapour is analogous to the specific heat of air. It ought, therefore, to increase in proportion as the particles of the vapour get further and further from each other. We have only to admit that the specific gravity of vapour increases with the elasticity or the temperature, to render the whole perfectly perspicuous. Now every thing conspires to satisfy us that this is really the case; but if we admit it, we can easily ascertain the specific gravity of vapour at every temperature. From the experiments of M. Gay-Lussac, it follows that the specific gravity of steam at 212° is 0·625, supposing the specific gravity of air at that temperature to be unity. From this, it is obvious, that if we reckon the specific gravity of air at 60°, 1·000, then the specific gravity of steam at 212° will be 0·472. Hence it appears that when water is converted into steam, its volume is increased 1754 times. Now this approaches very near to 1800 times, which was the increase of bulk determined long ago by the experiments of Mr. Watt.

The following little table exhibits the specific gravity of vapour at different temperatures both above and below 212°, calculated on the supposition that the specific gravity of vapour increases as its elasticity. The reader, by means of the tables of the elasticity of steam at different temperatures, which I have inserted in vol. i. p. 61, of the sixth edition of my System of Chemistry, may extend this little table as far as he thinks proper. The specific gravity of common air at 60° is reckoned unity.

Temperature.	Sp. gr. of aqueous vapour.
32°	0·00314
40	0·00413
50	0·00590
60	0·00824
70	0·01134
80	0·01440
90	0·02140
100	0·02880
212	0·472
250	0·944
300	2·203
343·6	3·776

I have entered into the preceding details, because the knowledge of them puts it in our power to determine the amount of the error occasioned by the gas, whose specific gravity we are determining, containing as much vapour as can exist in it under the given temperature.

Let us suppose that we determine the specific gravity of common air by weighing 50 cubic inches of it in a glass flask at the temperature of 60°. And let us suppose further that this portion of air is saturated with vapour from having been left for some time in contact with water.

At the temperature of 60°, the elasticity of vapour is 0·52 inch of mercury, and its specific gravity 0·00824; while that of air is 1·000. Now 0·52 is nearly 1-58th of 30. The problem, therefore, is reduced to finding the specific gravity of a mixture of 57 volumes of air of the specific gravity 1, and one volume of vapour of the specific gravity 0·00824.

Let A = volume of air = 57.
a = specific gravity of air = 1.
B = volume of vapour = 1.
b = specific gravity of vapour = 0·00824.
x = specific gravity of mixture.

Then, by a well known principle in pneumatics,

$$x = \frac{\mathrm{A}a + \mathrm{B}b}{\mathrm{A} + \mathrm{B}} = \frac{57 + 0{\cdot}00824}{58} = 0{\cdot}9829.$$

The presence of the vapour, it is obvious, diminishes the specific gravity of the air a little. The true specific gravity of air should have been 1·0000; but we have obtained only 0·9829, which is less by 0·0171, or somewhat more than 1-58th part.

If the temperature, instead of 60°, had been only 32°, the error would have been less. At that temperature, the elasticity of aqueous vapour is 0·2 inch, and its specific gravity 0·00314; so that the volume of air is 149, and that of vapour 1.

Here we have

$$\begin{aligned} \mathrm{A} &= 149 \\ a &= 1 \\ \mathrm{B} &= 1 \\ b &= 0{\cdot}00314 \end{aligned}$$

Consequently

$$x = \frac{\mathrm{A}a + \mathrm{B}b}{\mathrm{A} + \mathrm{B}} = \frac{149 + 0{\cdot}00314}{150} = 0{\cdot}99336,$$

or little more than 1-150th part below the truth.

We see from these examples, that the specific gravity of air is diminished very nearly by the volume of vapour mixed with it. And the lower the temperature, the more nearly does this approach to accuracy; because the specific gravity becomes always less and less considerable.

When the gas under examination is heavier than common air, the error becomes more considerable. The heaviest gas, whose specific gravity can be taken over water, is chlorine. Its specific gravity is 2·5. Let us determine the error, when we weigh it, standing over water at the temperature of 32°. Here we have

$$\begin{aligned} \mathrm{A} &= 149 \\ a &= 2{\cdot}5 \\ \mathrm{B} &= 1 \\ b &= 0{\cdot}00314 \text{ and} \end{aligned}$$

$$x = \frac{\mathrm{A}a + \mathrm{B}b}{\mathrm{A} + \mathrm{B}} = \frac{149 \times 2{\cdot}5 + 0{\cdot}00314}{150} = \frac{362{\cdot}50314}{150} = 2{\cdot}416687$$

Here the error is 0·083313, or 1-30th part.

When the gas is lighter than common air, the error diminishes; but still continues too great to be neglected. The lightest gas with which we are acquainted is hydrogen gas. Its specific gravity is $0{\cdot}069\dot{4}$. Let us determine what the specific gravity would be when weighed over water at the temperature of 32°. We have

$$\begin{aligned} \mathrm{A} &= 149 \\ a &= 0{\cdot}069\dot{4} \\ \mathrm{B} &= 1 \\ b &= 0{\cdot}00314 \text{ and} \end{aligned}$$

$$x = \frac{\mathrm{A}a + \mathrm{B}b}{\mathrm{A} + \mathrm{B}} = \frac{10{\cdot}347\dot{2} + 0{\cdot}00314}{150} = \frac{10{\cdot}35036}{150} = 0{\cdot}0690.$$

Here the error is 0·0004, or about 1-173d part; or the specific gravity of hydrogen gas, when taken in this way, comes out 1-173d part below the truth.

The method, which I am in the habit of adopting to obviate this source of inaccuracy, is very simple; and though it does not annihilate the error; yet it reduces it to so small a quantity, that it may be neglected without any bad consequences. The method is this:

The flask in which the gas is to be weighed is exhausted by the air pump, and then filled with common air, which has been standing for some time upon the same water trough with the gas whose specific gravity is to be taken. Thus filled with common air, it is weighed very accurately by means of a balance made for me by Mr. Crichton, of Glasgow, which when loaded with a pound in each scale turns sensibly with the 1-200th of a grain troy.* The flask is then exhausted and weighed again. Let the loss of weight be *m*. Finally, the flask is filled with the gas, whose specific gravity is wanted, and weighed again. Let the increase of weight be *n*. It is obvious that the specific gravity of the gas is $= \frac{n}{m}$.

Let us suppose that the specific gravity of pure hydrogen gas is taken in this way at the temperature of 32°. We have seen that at that temperature the hydrogen gas weighs 1-173d part less than it would do if it were dry, and that the common air weighs 1-150th less than the true weight. These two errors do not indeed balance each other exactly; but they reduce the error to 1-7th of what it would be, if we were to deduce the specific gravity of the hydrogen gas by comparing it with the weight of dry air; so that the deviation from the truth is reduced to $\frac{1}{1211}$ part. And by this the hydrogen gas will weigh more than it ought instead of less; for 0·9933 : 1 :: 0·0690 : 0·06946 = specific gravity of hydrogen gas thus deduced. Now this exceeds the true specific gravity of hydrogen gas by somewhat less than 2 in the fifth decimal place.

Now if we suppose the flask capable of holding 100 cubic inches of gas (and this exceeds the size of my flask), the hydrogen weighed would not exceed two grains. My balance is only capable of going to the 1-200th of a grain, or to the 4000th part of the weight of the hydrogen. Hence it is obviously incapable of determining the weight of the hydrogen gas to the fifth decimal place with accuracy. On that account I never go further than four decimal places; so that an error in the fifth is of no consequence.

Let us see what the error would amount to if we take the specific gravity of hydrogen gas in this way at the temperature of 60°.

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* This balance does Mr. Crichton's skill a great deal of credit. It is the best balance for chemical purposes which I have ever seen.

Here $A = 57$

$$a = 0{\cdot}0694$$
$$B = 1$$
$$b = 0{\cdot}00824 \text{ and}$$

$$x = \frac{A\,a + B\,b}{A + B} = \frac{3{\cdot}9583 + 0{\cdot}00824}{58} = \frac{3{\cdot}96657}{58} = 0{\cdot}06838.$$

Here the error is 0·0106, or almost 1-65th part, by which the hydrogen gas is too light. But, at the same temperature, common air saturated with moisture is about 1-58th part too light. These two errors do not quite correct each other; but they render the specific gravity of the hydrogen gas somewhat higher than the truth; for 0·9829 : 1 :: 0·06838 : 0·06956 = sp. gr. of hydrogen gas taken in this way. This exceeds the truth about 1-600th part. We see from it that the specific gravity of hydrogen gas should be taken at as low a temperature as possible.

I have little doubt that the specific gravity of hydrogen gas found by Berzelius and Dulong; namely, 0·0688, was a little too light, in consequence of the presence of the vapour of water in it. To prevent the vapour of water from mixing with their gas, they covered the surface of the water in the trough with oil. But if the gas, when produced, passed through water, as it obviously must have done, this precaution could not have answered the end intended. It is obvious that the presence of vapour, instead of augmenting the specific gravity of the gases, would have diminished it. The error then in the determination of the specific gravity of hydrogen gas by Biot and Arago, did not proceed from the presence of vapour; but from the presence of a small quantity of common air.

ARTICLE VIII.

Observations on the Temperature of Mines in Cornwall. By Mr. M. P. Moyle.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Helston, March* 16, 1822.

MR. FOX having communicated to the editors of the Annales de Chimie et de Physique new determinations on the temperature of the earth at various depths, these gentlemen have published them, along with an extract made by M. Fourier, from his profound geometrical researches on heat.

Mr. Fox's observations were made in 10 different mines in this county from the depth of 10 fathoms to that of 240 fathoms, at intervals of 10 fathoms from each observation; and, according to his descent, it appears that the temperature of the earth gradually increased from that of 50·18° Fahr. at 10 fathoms to that of 82·04° Fahr. at 240 fathoms, the bottom of Dolcoath Mine. This statement of Mr. Fox differing so entirely from a few observations which I made a few years since on the temperature of mines, compels me to notice them, and more particularly when Mr. F. would wish to infer the great superiority of temperature of the internal part of the earth over that of the surface. At the bottom of the mine at Dolcoath, 240 fathoms deep, there issues from the vein a jet of water, whose *constant* temperature is 80·04° Fahr. "What more evident proof can be given," says Mr. Fox, "of the great heat of the interior state of the globe?" Surely Mr. F. would not infer from this the superior heat of the internal strata *generally;* he might as well draw his conclusions from measuring the temperature of the boiling fountain in Iceland, which spouts its columns to the height of 90 feet, and is found boiling-hot after its descent. The source of *this* heat it is not necessary to discuss; but I am apprehensive that Mr. F. would not have found the temperature of the earth at the same depth, and some way distant from the spring, so high.

I have taken the temperature of several different mines at various depths, and in the working part of mines have generally found the increase of temperature in a similar ratio to what Mr. Fox states; the cause of which I inferred was from the presence of so numerous a body of workmen in different parts of the said mine, often amounting to 400 or more, at one time, under ground, and generally the greatest number at the bottom; also from the greater confinement and density of the air. Surely this must have a great effect in not only warming the atmosphere of the spot, but the very walls of the galleries, and even their beds, to the depth of many inches; and although Mr. F. may have taken his observations when the bulb of the thermometer has been "placed six or eight inches in the body of the rock," he must not forget that the surrounding atmosphere must have penetrated to that depth before he could possibly have placed the thermometer there.

I come now to state a few of the results of my own observations. It does not appear from Mr. Fox's account that any part of a mine remote from the working had been proved, where we certainly should expect to find the medium, or rather the true mean, if any where. This I have done in several instances: one or two may suffice to convince the candid reader that Mr. Fox must either have drawn false conclusions, or did not take the temperatures in a proper manner.

Some years since in Wheal Unity (the same mine which Mr. Fox visited), one of the galleries to the western part of the mine, at the depth of about 150 fathoms, which had not been worked for more than 12 months, at the extreme end, there being no current, I found the temperature was just 65°, while the working part at the same depth was 74°.

In Wheal Trumpet Tin Mine, the extreme eastern part at 75 fathoms in depth, has not been worked for 18 months. This gallery has no other communication with any other part of the mine for a distance of more than 20 fathoms in length: here the temperature was two months since 52°; the working part, 30 fathoms distant, at the same time, and at the same depth, was 67°, the temperature of the open atmosphere being 60°. At the 86 fathom gallery in this mine, the water that issues from the vein was 51°, while the air of the same place was 68·7°.

I have also proved the temperature of several old mines which have ceased working for many years. At the adit level of Old Trevenen Tin Mine (14 fathoms from the surface), the temperature was less by 4° than the common atmosphere. This most probably may be owing to the stillness of the air, and not being subject to such quick variations of temperature as on the surface. A shaft in this mine being full of water from the bottom to the adit level, the water proved 2·5° lower than the atmosphere at the surface, which, in my opinion, clearly proves that had the bottom part of this mine (about 110 fathoms) been much warmer than the surface of the earth, its heat would, in the course of eight years, which is the time since she ceased working, have been communicated to this water generally, especially as this shaft is always overflowing, and in which case it would be indicated by the thermometer.

I might adduce more instances to prove what I have here asserted, but I conceive sufficient has been said to show that Mr. Fox could only have tried places in which the air was influenced by the presence of the workmen. I can also prove that considerable variation in the temperature of a part of a mine is caused by the different currents of air, being in some places very still and confined, and in others, a few feet distant, so strong that a candle is constantly blown out.

I am, Sir, your humble servant,

M. P. Moyle.

Article IX.

Analyses of Books.

Mémoires de la Société de Physique et d'Histoire Naturelle de Genève. Tom. I. Première Partie.

We are informed in the preface to this work, that although the Physical Society of Geneva originated in 1790, yet the greater part of the Memoirs which had been read before it have been gradually published by their authors in scientific journals, or other works. The Society being, however, of opinion, that several communications which they possessed were worthy of publication, a part of them has now been printed; and for this we are indebted to the present volume, or rather the first part of a volume.

The names of those who constitute the Society are sufficient to raise expectations that the subjects treated of will be of such a nature as to interest the scientific reader; and we think it will be allowed, on perusing the memoirs contained in this volume, that the Society has made a judicious selection of the communications presented to them.

These Memoirs are 12 in number, and, for the present, we must content ourselves with enumerating them, intending to take an early opportunity of making such extracts from the more interesting, as may convey some idea of their respective merits.

I. Memoir upon some Peculiarities in the Eye of the Tunny (Scomber Thynnus of Linnæus), and some other Fishes. By Mr. L. Jurine.

II. Notice respecting the Teeth and Mastication of the Fishes called Carp. By the Same.

III. On the Effect of the Motion of a refringent Plane upon Refraction. By Mr. P. Prevost.

IV. Observations upon the Relations which exist between the Axes of Double Refraction, and the Form of Crystals. By Mr. F. Soret.

V. Notice respecting Mica. By the Same.

VI. Memoir on different Physical and Meteorological Instruments. By Mr. Peter Huber.

VII. Memoir on the Fall of Leaves. By Mr. P. Vaucher.

VIII. Notice relating to the Basaltic Country of the Departments of the Rhine, Moselle, and Sarre. By Mr. M. A. Pictet.

IX. Memoir on the *Charagnes*. By Mr. Vaucher.

X. Essay on the Spermatic Animalculi of several Animals. By MM. J. L. Prevost and J. A. Dumas.

XI. Memoir on the Natural Affinities of the Family Nymphæa. By P. M. De Candolle.

XII. On the Influence of Green Fruits upon the Air before they ripen. By Mr. De Saussure.

Article X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Feb. 28.—Communication of a curious Appearance lately observed upon the Moon. By the Rev. F. Fallows.

On the Difference in the Appearance of the Teeth and Shape of the Skull in different Species of Seals. By Sir Everard Home, Bart.

March 7.—Experiments and Observations on the Developement of Magnetical Properties in Steel and Iron by Percussion. By William Scoresby, Jun. (Communicated by the President.)

March 14 and 21.—A paper was read on the Alloys of Steel. By J. Stodart, Esq. FRS. and Mr. Faraday, Chemical Assistant to the Royal Institution.

These alloys were first made on a small scale in the laboratory of the Royal Institution. The results proving satisfactory, the experiments were extended, and alloys made for the purpose of manufacture to a considerable extent; these proved equal, if not superior, to the smaller productions of the laboratory.

The metals that formed the most valuable alloys with steel were stated to be silver, platinum, rhodium, iridium, osmium, and palladium, and, with the exception of silver, the best proportion of the alloying metal about 1-100th part. Steel with silver will combine with only 1-500th part; when more is fused, the metals form only a mechanical mixture. These alloys may be advantageously used for every purpose where good steel is required, but the scarcity and value of some of the metals must operate as a preventive to their general introduction.

The experimentalists were most liberally furnished with all these metals through the kindness of Dr. Wollaston.

The presence of the alloying metal in the alloy was constantly proved by chemical tests, and the compound, after being forged into a bar, was further examined as to uniformity, by acting on the surface previously brightened by diluted acid.

Such processes of analysis were given as were deemed useful to the manufacturer, the general process was to act by dilute sulphuric acid, to burn off the carbon &c. from the residuum, and then examine the matter left by the means generally required for each particular metal. A remarkable fact was noticed as to the promptness of action exerted by acids on some of the alloys; those, for instance, containing platinum, and some other metals being acted on many times more rapidly by acids than unalloyed steel.

The action of acids on hard and soft steel was found also to leave residua very different in kind; that from hard steel being a black carbonaceous powder; while that from soft steel and soft alloys was in much greater abundance and plumbagenous.

When the alloys were acted on by dilute sulphuric acid, the residuum boiled in the acid, and the powder left acted on by nitric acid, this powder, whenever the alloy contained a metal insoluble in nitric acid, was either detonating or strongly deflagrating; whereas, when the alloying metal was soluble in nitric acid, the powder was entirely dissolved, and nothing of a similar nature produced.

It was observed that the metals platinum and rhodium combine with steel in every proportion, forming with some of the higher proportions beautiful compounds, the colour favourable for metallic mirrors, and not subject to tarnish on exposure to the atmosphere. Steel with the last named metal was particularly noticed.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

The following papers have been read since the last Report:

On the Mineral Productions and Geology of the Parish of St. Just. By Joseph Carne, Esq. FRS. MRIA. Member of the Society.

On some Advantages which Cornwall possesses for the Study of Geology, and on the Use which may be made of them. By John Hawkins, Esq. FRS. Honorary Member of the Society.

On Stratification, and on the external Configuration of the Granite of Cornwall. By John Forbes, MD. Secretary of the Society.

On the Gwithian Sands. By Henry Boase, Esq. Treasurer of the Society.

On the Slaty Rocks of Cornwall, more particularly on those usually denominated Killas. By Dr. Forbes.

Additional Observations on the Temperature of Mines. By R. W. Fox, Esq. Member of the Society.

Notice on the Geology of Nice. By G. C. Fox, Esq. Member of the Society.

Some Account of the South American Mines. By the Rev. John Trevenen.

Some Account of the Mines of Pasco, in South America. By Mr. Richard Hodge. Communicated, with additional Observations, by Sir Christopher Hawkins, Bart. MP. FRS. Member of the Society.

Some Account of the external Features (natural and artificial) of a Country, from which its Geological Structure may be inferred. By Dr. Forbes.

Notice of the Quantity of Copper raised in Great Britain and Ireland in the Year ending June, 1821. By Mr. Alfred Jenkyn, Member of the Society.

Notice of the Quantity of Tin raised in Cornwall in the Year ending June, 1821. By Joseph Carne, Esq. FRS.

Officers and Council for the present Year.

President.—Davies Gilbert, Esq. MP. VPRS. &c. &c.

Vice-Presidents.—Sir C. Hawkins, Bart. MP.; Sir R. H. Vivian, KCB.; J. H. Tremayne, Esq. MP.; HP. Tremenheere, Esq.

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Article XI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Edward Daniel Clark,* LLD. FRS. &c. &c.

In announcing the lamented death of this distinguished philosopher and traveller, the editor is permitted to state, that a biographical notice by one of his intimate friends perfectly competent to appreciate his merits in every branch of science, will appear in the next number of the *Annals of Philosophy.*

II. *Precipitation of Silver by Chlorine.*

The editor of the Annales de Chimie et de Physique, Vol. XVIII, p. 270, alluding to a statement made by Mr. Faraday and myself, that a gas was chlorine, because it precipitated nitrate of silver, says in a note, "This gas could not be pure chlorine, for it would not precipitate nitrate of silver; it must have contained hydrochloric acid."

It is difficult to account for this mistake, and still more difficult to suppose that it could have originated with either of the acknowledged editors of the Annales de Chimie; to prove its incorrectness, it is requisite, only to refer to the tenth volume of the same work, p. 425, and eleventh volume, p. 108,—M. Gay Lussac there states, that if nitrate of silver be dropped into a solution of chloride of lime, until no further precipitation takes place, the supernatant liquid, if moderately heated, is decomposed, and oxygen gas disengaged; and if the residual matter be dissolved in water acidulated with nitric acid, a portion of chloride of silver remains behind. The fact is, that a portion of chlorate of silver is formed which prevents the precipitation of the whole of the silver in the state of chloride; but it is evident from this very statement, that when chlorine gas is passed into nitrate of silver, a portion of chloride must be precipitated.

Nitrate of silver must, therefore, be considered as a test of the presence of chlorine, even when uncombined with hydrogen; and it is also evident, that nitrate of silver cannot be relied upon for determining the quantity of free chlorine: nor did Mr. Faraday and myself attempt to employ it for this purpose.—*Edit.*

III. *Composition of Oxalic Acid.*

M. Dobereiner stated about five years since, that oxalic acid contains no hydrogen, and that it is formed of equal volumes of oxide of carbon and carbonic acid, combined with a proportion of water. He considered this water as essential to its existence, and that if it were taken away, the acid would be decomposed. Reflecting afterwards upon the great affinity which fuming sulphuric acid has for water, he performed the following experiment, which he has described in a pamphlet upon pneumatic chemistry:

Five grains of dried oxalic acid, but still containing a quantity of water, were mixed with 200 grains of fuming sulphuric acid, in an apparatus for receiving gases over mercury. The oxalic acid gradually and totally disappeared, and produced 9·4 cubic inches of gas; the sulphuric acid became less fuming.

The gases washed with ammonia were reduced to 4·7 cubic inches, and consequently contained 4·7 cubic inches of carbonic acid. The gas which the ammonia did not absorb was oxide of carbon; for it burnt with a blue flame, and being detonated in Volta's eudiometer with half its volume of oxygen, it produced an equal volume of the carbonic acid, without any appearance of water; the weight of carbonic acid, added to that of the oxide of carbon, represents exactly that of the anhydrous oxalic acid; and M. Dobereiner concludes that this acid contains no hydrogen; for if it contained any, sulphurous acid should be formed; or if the hydrogen was combined with a portion of oxygen of the oxalic acid, the carbonic gas and the oxide of carbon would be found in different proportions.

In this experiment, the sulphuric acid combines only with the water, and in order that it may succeed, it is requisite that the sulphuric acid should be fuming, like that of Nordhaussen: for common acid does not decompose oxalic acid.—(Ann. de Chimie et Phys.)

IV. *Hot Springs of St. Michael.*

The vicinity of the springs is indicated by the increased temperature of the earth, a sulphurous odour, and the escape of vapour or steam from every crack or fissure in the ground. The volumes of smoke and steam rolling upwards from the surface to a great height, till they are gradually diffused through the atmosphere, or mingle with the heavier clouds that crown the summit of the mountains, produce a striking effect. The confused rumbling and hissing noise that is heard for some time before we arrive in sight of the springs, increases at last to an incessant and terrific roar, and seems to issue from the very spot on which we stand. The earth returns a hollow sound, and great caution is required to avoid stepping into the pools and streams of boiling water with which its surface is covered.

The quantity of hot water discharged through the innumerable orifices in the ground is prodigiously great, and the different streams unite, forming a small river, that, still hot, joins the Ribeira Quente. The largest streams are termed "caldeiras," or boilers, and a shallow basin of earthy matter has been formed round each of them by depositions from the water. Much of the water is constantly restrained within these reservoirs, and its surface is more or less agitated by the escape of sulphuretted hydrogen gas, and the ejection of water from below. The temperature of some of these springs on the second day of December, between three and four o'clock, p. m., the thermometer standing at 63 degrees, Fahrenheit; the barometer at 29·4, was as follows:—

207°	200°	98°	137°	203°
190	134	170	73	114
184	94	122	171	147*

The basin of the largest spring particularly designated as "the Caldeira," is circular, and between twenty and thirty feet in diameter. The water in this boils with much greater violence than in any other Caldeira, and distinct loud explosions occur at short intervals, which are succeeded by a very perceptible elevation of the centre of the body of water within the basin. This is attended with a loud hissing noise, the escape of great quantities of sulphuretted hydrogen gas, steam, and sulphurous acid vapour. On account of the high temperature, and vast quantities of steam, it is dangerous to approach near this spring, except on the windward side. The cattle, however, are often seen standing on the opposite side, to free themselves, as it is supposed, from vermin. The peasants are in the habit of placing baskets filled with lupines, beans, and other vegetables, on the edge of the basin where they are speedily cooked.

Every interstice in the ground, and the surfaces of many of the loose rocks, are incrusted with sulphur, which is sometimes crystallized in acute pyramids, but more commonly in delicate fibres. A considerable quantity of it might be collected in a short time. Silex is deposited from the water under a variety of forms, and many small pieces of pumice and altered lava are cemented by it.—(Dr. Webster's History of the Island of St. Michael.)

V. *On the Solution of Carbonate of Lime.*

Mr. Dalton, in a paper containing remarks on the analysis of spring and mineral waters, has stated some interesting circumstances respecting the alkaline properties of solution of carbonate of lime. It is stated, "that all spring water containing carbonate or super-carbonate of lime, is essentially limy or alkaline, by the colour tests. And this alkalinity is not destroyed till some more powerful acid, such as the sulphuric or muriatic is added, sufficient to saturate the whole of the lime. Indeed, these acids may be considered as sufficient for tests of the quantity of lime in such waters; and nothing more is required than to mark the quantity of acid necessary to neutralize the lime. It does not signify whether the water is boiled or unboiled, nor whether it contains sulphate of lime along with the carbonate; it is still limy in proportion to the quantity of carbonate of lime it contains. Agreeably to this idea, too, I find that the metallic oxides, as those of iron or copper, are thrown down by common spring water, just the same as by free lime, notwithstanding, this carbonate of lime, in solution in water, contains twice the acid that chalk or limestone does. I fully expected the super-carbonate of lime in solution

* The above are put down in the order in which they were examined.

to be acid; but it is strongly alkaline, and scarcely any quantity of carbonic acid water put to it, will overcome this alkalinity. Pure carbonic acid water is, however, acid to the tests. I could not be convinced of the remarkable fact stated in this paragraph, till I actually formed super-carbonate of lime, by super-saturating lime water in the usual way, till the liquid from being milky became clear. It still continued limy, and was even doubtfully so when two or three times the quantity of acid was added. It should seem, then, to be as impossible to obtain a neutral carbonate of lime, as it is to obtain a neutral carbonate of ammonia, in the sense here attached to the word neutral."—Memoirs of the Manchester Society.

Article XII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

The Naturalist's Guide, or Directions for the collecting and Preservation of Animals and Plants. By William Swainson, Esq.

Practical Observations on Paralytic Affections, Deformities of the Chest and Limbs, illustrative of the Effects of muscular Action. By Mr. W. T. Ward.

A System of Analytic Geometry. By the Rev. Dionysius Lardner.

The Fossils of the South Downs, or Illustrations of the Geology of Sussex. By Gideon Mantell, FLS. In one Vol. royal 4to. with numerous Engravings.

JUST PUBLISHED.

Memoirs of the Astronomical Society of London. Vol. I. 4to. 1*l.* 1*s.*

A Universal Technological Dictionary of the Terms used in all Arts and Sciences. By George Crabb, AM. Illustrated with numerous Cuts, Diagrams, and Plates. 4to. Parts. I and II. 9*s.* each. To be completed in 12 Monthly Parts.

Travels in the Interior of Southern Africa. By W. J. Burchell, Esq. With an entirely new large Map, numerous coloured Engravings, and 50 Vignettes, from the Author's Original Drawings. 4to. Vol. I. 4*l.* 14*s.* 6*d.*

An Essay on the Uterine Hæmorrhage, which precedes the Delivery of the Full-grown Fœtus; illustrated with Cases. By Edward Rigby, MD. FLS. FHS. &c. 8vo. 7*s.*

A Series of Questions and Answers, for the Use of Gentlemen preparing for their Examination at Apothecaries' Hall; with copious and useful Tables annexed. By Charles Mingay Syder. 4*s.*

The Chemical Decompositions of the London Pharmacopeia. By Charles Mingay Syder. 18mo. 1*s.* 6*d.*

Remarks on Cutaneous Diseases. By J. H. Wilkinson. 8vo. 3*s.* 6*d.*

The Florist's Directory, a Treatise on the Culture of Flowers. By James Maddock, Florist. 8vo. Plain, 12*s.*; coloured, 20*s.*

Article XIII.

NEW PATENTS.

C. Broderip, Esq. of London, now residing in Glasgow, for various improvements in the construction of steam-engines.—Dec. 5, 1821.

H. Ricketts, of Phœnix Glass Works, Bristol, glass manufacturer, for an improvement in the art or method of making or manufacturing glass bottles, such as are used for wine, porter, beer, or cyder.—Dec. 5.

W. Warcup, of Dartford, Kent, engineer, for certain improvements upon a machine for washing linen, cotton, or woollen cloths, whether in the shape of piece goods, or of any article made up.—Dec. 10.

W. Horrocks, of Portwood-within, Binnington, in the county of Chester, cotton-manufacturer, for an improvement in the construction of looms for weaving cotton or linen cloth by power, commonly called Power Looms.—Dec. 14.

J. Winter, Gent. of Stoke-under-Hamdon, Somersetshire, for certain improvements in a machine for sewing and pointing leather gloves with neatness, much superior to that which is effected by manual labour.—Dec. 19.

S. Brierley, of Salford, Manchester, dyer, for an improved method of preparing raw silk, and cleansing the same, for the purpose of dyeing and manufacturing.—Dec. 19.

J. Gladstone, of Castle Douglas, in the stewatry of Kircudbright, and county of Galloway, engineer and mill-wright, for an improvement or improvements in the construction of steam-vessels, and mode of propelling such vessels by the application of steam or other powers.—Dec. 20.

Julius Griffith, Esq. of Brompton Crescent, for certain improvements in steam-carriages, and which steam-carriages are capable of transporting merchandise of all kinds, as well as passengers, upon common roads, without the aid of horses. Partly communicated to him by foreigners residing abroad.—Dec. 20.

Pierre Erard, of Great Marlborough-street, musical instrument maker, for certain improvements on pianofortes, and other keyed musical instruments. Communicated to him by a foreigner.—Dec. 22.

G. Linton, of Gloucester-street, Queen-square, Middlesex, mechanist, for a method of impelling machinery without the aid of steam, water, wind, air, or fire.—Dec. 22.

R. Ormrod, of Manchester, iron-founder, for an improvement in the mode of heating liquids in boilers, and thereby accelerating and increasing the production of steam. Communicated to him by a person residing abroad.—Jan. 7, 1822.

R. S. Harford, of Ebbro Vale Iron Works, Aberistwyth, iron-master, for an improvement in that department of iron commonly called puddling.—Jan. 9.

J. Harris, of St. Mildred's-court, London, tea-dealer, for an improvement in the manufacture of shoes for horses and cattle.—Jan. 9.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1822.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
2d Mon.								
Feb. 1	S W	30·28	29·93	48	35	—		3°
2	S W	29·93	29·54	54	44	—	27	3
3	S W	29·88	29·55	47	31	—		
4	S W	29·88	29·41	49	36	—	12	
5	S W	30·26	29·45	52	30	—	01	6
6	W	30·26	29·99	45	32	—		20
7	S W	29·99	29·92	51	43	—	20	4
8	S W	29·98	29·96	50	42	—		11
9	S W	29·99	29·97	53	41	—		3
10	S E	30·09	29·99	54	38	—		
11	S W	30·31	30·09	51	35	—		16
12	N W	30·31	30·25	43	34	57		
13	S E	30·25	30·18	50	36	—		10
14	S E	30·18	30·16	50	31	—		4
15	S	30·42	30·15	54	35	—	07	14
16	W	30·44	30·42	48	37	—		12
17	W	30·44	30·44	52	40	—		
18	S W	30·46	30·40	56	41	—	02	9
19	N W	30·46	30·14	51	36	—		6
20	S W	30·41	30·06	48	32	—	11	
21	N	30·49	30·41	46	26	44		14
22	S W	30·41	30·38	50	36	—		11
23	S W	30·38	30·27	50	39	—		
24	S W	30·31	30·28	55	48	—		
25	N W	30·31	30·21	55	44	—		5
26	W	30·56	30·21	52	36	—	02	
27	N W	30·70	30·56	48	24	—		15
28	S W	30·76	30·36	50	22	57		18
		30·76	29·41	56	22	1·58	0·82	20—3

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Second Month.—1. Fine. 2. Cloudy: a very stormy night, the wind blowing a gale the greater part of it. 3. Very fine. 4. Cloudy and fine. 5. Very windy all day: a heavy storm of wind and rain about six, a.m. 6. Fine: lunar halo. 7, 8, 9. Cloudy. 10, 11. Fine. 12. Cloudy. 13. Foggy morning: fine day. 14. Fine. 15. Fine: rain at night. 16. Fine. 17. Cloudy morning: fine day. 18, 19. Cloudy. 20. Cloudy: rain in the evening. 21. Cloudy and fine. 22. White frost. 23. Fine. 24. Cloudy. 25, 26, 27. Fine. 28. Hoar frost: fine.

RESULTS.

Winds: N, 1; SE, 3; S, 1; SW, 15; W, 4; NW, 4.

Barometer: Mean height	
For the month	30·193 inches.
For the lunar period, ending the 14th	30·159
For 13 days, ending the 6th (moon north)	30·095
For 14 days, ending the 20th (moon south)	30·209
Thermometer: Mean height	
For the month	43·321°
For the lunar period	41·583
For 29 days, the sun in Capricorn	42·655
Evaporation	1·58 in.
Rain	0·82

Laboratory, Stratford, Third Month, 21, 1822. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1822.

Article I.

Anatomical Discoveries respecting the Organ of Hearing in Fishes.

In the year 1820, a thin quarto volume was published at Leipsic by Dr. Ernest Henry Weber, entitled "De aure et auditu Hominis et Animalium, pars I. De aure Animalium Aquatilium." By this first part, Dr. Weber considers the following new facts to be the results of his anatomical labours on fishes. We, therefore, insert a translation of them here in order to draw the attention of our comparative anatomists to the anatomy of the ear of fishes.*

1. The *petromyzontes* (*lampreys*), both of rivers and the sea, are furnished with a cartilaginous vestibulum separate from the cavity of the cranium, but they are destitute of semicircular canals, both cartilaginous and membranaceous. They are likewise destitute of lapilli enclosed in the vestibulum, or in a bursa, and have no external organs of hearing. Their membranaceous vestibulum is divided into different cells.

2. In several genera of osseous fishes, and especially of the order abdominales, the swimming bladder is joined in a particular way with the internal ear, and is useful to the membrana tympani.

3. This conjunction of the swimming bladder with the internal ear in the cyprinus carpio (*common carp*), brama (*bream*), tinca (*tench*), carrassius (*crusian*), rutilus (*roach*), aphyas, leuciscus (*dace*), alburnus (*bleak*), and doubtless in all the cyprini; like-

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*** We have not ventured to translate the Latin names by which Weber distinguishes the parts which he describes, conceiving them likely to be more generally intelligible than the corresponding English ones.**

wise in the silurus glanis and cobitis fossilis and the barbatula, is accomplished by means of six ossicula auditoria (three of which are placed on the right side and three on the left) united with the three superior vertebræ by articulation. These may be compared to the stapes, incas, and malleus. The apex of the malleus always adheres to the upper part of the swimming bladder.

4. All the fishes just enumerated are furnished with two cavities (*atria*) situated in the first vertebra near the foramen occipitale. Each cavity is shut by the stapes of that side in which it is placed; and the stapes may either be drawn from it, or applied to it, by the action of the swimming bladder. Hence this cavity may be compared to the fenestra ovalis in man. Each cavity (*atrium*) is furnished with a little bone peculiar to itself which shuts it up.

5. In all the fishes above enumerated, each cavity (*atrium*) has access to the sinus imparis by means of two holes cut in the occipital bone. This sinus is situated in the middle part of the basilary portion of the occiput. Passing into the cranium like a fork, it is divided into two canals, of which the right passes to the right labyrinth, and the left into the left labyrinth, to which it adheres in that place in which the saccus and vestibulum membranaceum are united.

6. In all the fishes above enumerated, there are found certain *ostia* leading into the same cavity of the cranium, covered with skin and muscles, which must be considered as the fenestræ of the osseous vestibulum, since the cranium of osseous fishes serves the same purpose as the vestibulum osseum.

7. In all these fishes, the three superior vertebræ receiving the ossicula auditoria are increased, augmented in size, and remarkably altered.

8. All these fishes possess an interior lapillus of the sack, remarkable by a peculiar form, long, and spinous.

9. The ossicula auditoria of the cyprinus are enclosed by two membranous auditorial fossæ, one of which is situated on the right side, and the other on the left side of the three superior vertebræ. The fossæ auditoriæ communicate with the cavity of the cranium by the two lateral occipital bones, and contain an oily liquor of the same nature as that in the cranium.

10. The ossicula auditoria of the cobitis fossilis are included in a cavity of the transverse process of the second vertebra, answering the purpose of the cavity of the tympanum.

11. The osseous capsule, enclosing the swimming bladder of the cobitis fossilis is formed from the transverse processes of the third vertebra, expanded into an osseous bulla. This capsule has two great external apertures, surrounded outwardly by an elevated margin, covered by the external cutis. By two other anterior openings, the apex of the malleus of the right and left side enters into the osseous capsule, and is there fixed to the

swimming bladder. This osseous capsule answers the same purposes as the annulus of the tympanum in the human infant. Consequently sonorous tremors have access to the swimming bladder through the apertures covered with skin; from which they are transferred by means of the malleus, incus, and stapes, to the membranous labyrinth.

12. This conjunction of the swimming bladder and internal ear is brought about in other fishes not by ossicula auditoria, but so that canals from the swimming bladder ascend directly to the head, and are joined to the ear in different ways.

13. In the sparus, salpa, and sargus, the top of the swimming bladder ascends to the base of the cranium divided into two canals. A peculiar membrane unites the apices of these canals to the margins of two oval bones situated on the right and left sides of the base of the cranium.

14. In the clupea harengus (*herring*), two very narrow canals of the swimming bladder enter into two bony canals on the right and left side of the base of the occipital bone. Each of these canals is again divided into two small bony canals whose extremities swell out into hollow bony globules, the anterior and posterior. The canals of the swimming bladder fill up these bony canals and globules. The appendix of the membranaceous vestibulum enters into the right and left anterior bony globule near the bullous end of the swimming bladder; so that reaching the extremity of the swimming bladder, it forms a septum, which separates the cavity of the appendix of the vestibulum filled with water from the cavity at the extremity of the swimming bladder filled with air. The circumference of this septum is surrounded by a ring nearly cartilaginous. Hence in the herring the sonorous tremors of the swimming bladder are transferred to the membranous vestibulum itself.

15. The anterior part of the right membranous vestibulum of the herring communicates with the anterior part of the left membranous vestibulum, by means of a transverse membranous canal passing below the cerebrum, in such a manner that mercury cannot be injected into either vestibulum without filling at the same time the membranous vestibulum and the semicircular canals of the opposite side.

16. The lower end of the swimming bladder of the herring and anchovy is produced into a canal situated between the two ovaries, and behind the rectum which opens into the ostium genitale.

17. The swimming bladder of the cobitis fossilis is not simple, but consists of two parts, the upper greater, and the lower, very small, situated without the bony capsule. A fibrous process passes from the skin to the swimming bladder by an external opening of the osseous capsule.

18. The canal for air (*canalis pneumaticus*) of the swimming bladder of the cyprini entering into the œsophagus cannot be

opened and shut by means of a valve, but forms a muscular tumour, through which the air canal penetrates in a spiral direction, diminished to the fourth part of its size.

19. The ear of the ray fish is not furnished with a single external passage, as all anatomists have hitherto believed; but with two. Besides the fenestra of the vestibulum cartilagineum closed by a membrane, described by Scarpa and Cuvier, there exists a fenestra of the vestibulum membranaceum situated beside it. The fenestra of the vestibulum membranaceum is to be compared to the fenestra ovalis in man, and the fenestra of the vestibulum cartilagineum to the fenestra rotunda in man. The fenestra of the vestibulum cartilagineum forms an opening into the cavity of the vestibulum cartilagineum, the fenestra of the vestibulum membranaceum in like manner forms an opening into the cavity of the vestibulum membranaceum.

20. Between the fenestræ of the vestibula membranacea cut out in the cartilaginous cranium (belonging to the right and left ear) and the skin covering the occiput, two bags are interposed filled with a white calcareous liquor, and touching each other. From each of these, a large membranous canal entering through the fenestra of the vestibulum membranaceum, descends into the vestibulum membranaceum, and fills its cavity. These bags called by Weber the sinuses of the external ear, and compared by Monro to the conchæ of the human ear, answer the purpose of the cavity of the tympanum, and the liquor included in them serve the purpose of the bones of the ear.

21. One or more very small canals, detected by Monro; but not found by Camper, Scarpa, Comperetti, and Cuvier, pass from the auditory sinus of each side to the cutis, and there open by very small mouths. By these canals, any excess of the calcareous liquid contained in the sinus auditorius may be thrown out.

22. There is a small muscle belonging to each auditory sinus, by means of which the auditory sinus may be compressed, and its liquor either thrown out through the small apertures opening in the cutis, or impelled through the canal into the membranous vestibulum. In this way the vestibulum membranaceum may be either compressed or relaxed.

23. The vestibulum membranaceum of the raia sarpedo marmorata (Risso) does not contain white cretaceous lapilli; but a gelatinous mass, mixed with a black coloured sandy matter.

24. The membranous semicircular canals of the raiæ are joined to each other and to the vestibulum membranaceum in quite a different manner from those of the squalus carcharias (*white shark*). The semicircular canals of the squalus carcharias have a semicircular form, while that of the raja has a circular form. The semicircular canals of the squalus carcharias proceed by one extremity from the vestibulum membranaceum, and by the other extremity return into it; whereas those of the raiæ are quite

separated from it, and indeed have no communication with it except by two very small ducts. One of these ducts passes from the vestibulum membranaceum to the posterior canal, which has the form of a circle, and does not cohere to the remaining canals; the other passes to the anterior canal.

25. The observation of Treviranus that the auditory nerves are not always to be considered as branches of the trigeminus is proved to be true.

26. The nervi auditorii accessorii have different origins in different fishes; proceeding from the cerebrum, the par vagum, and the trigeminus. Nor are these nerves always destined to the same parts of the labyrinth. In the raia torpedo, the squalus carcharias and the petromyzon (*lamprey*), the nervi auditorii accessarii do not belong to the ear. In several of the cyprini, the author describes a very remarkable deviation in these nerves.

27. The branches of the nervi auditorii belonging to the vestibulum are *soft*, and, as it were, spread over its inferior surface. The branches sent to the ampullæ are *hard*, and penetrate into the cavity of the ampullæ, constituting a semilunar plexus jutting out into the cavity. The sonorous tremors of the fluid included in the semicircular canals are readily transferred to these nerves. The nerves of the vestibulum receive the sonorous tremors of the solid bodies included in the vestibulum or sack.

ARTICLE II.

On the Annlysis of Brass. By Mr. W. M. Keates.

(To the Editor of the *Annals of Philosophy*.)

SIR,

HAVING noticed that most chemical writers, who have made mention of brass, state the proportions of the two metals which compose that alloy, to be very different from what they really are, I was induced to attribute the incorrectness of their statements to some fallacy in the mode of analyzing it; this opinion was verified by some experiments which I made on the subject.

The formula recommended by the most eminent writers is solution of the alloy in dilute nitric acid, adding excess of caustic potash, boiling, to take up the oxide of zinc, and throw down the oxide of copper.

This method of proceeding seems to have been followed from a consideration of the known habitudes of each oxide when *per se*, rather than founded on the result of direct experiment. For although the oxide of zinc, when alone, is readily soluble in an excess of caustic alkali, yet when precipitated along with oxide of copper, a part only is taken up by that menstruum.

This probably arises from some mechanical combination of the two oxides, and not from any chemical union. The following experiment may, I think, be considered as sufficient proof of the fallacy of the above mode of analysis.

(A.) 100 grains of brass dissolved in diluted nitric acid; the solution was heated until it was nearly neutral, and diluted with distilled water.

(B.) 540 grains of caustic potash dissolved in six ounces of water, were added to the solution A in a flask capable of holding 36 fluid ounces, using agitation during the mixture. The flask was placed on a sand-bath, and the contents boiled for an hour and a half, precautions being taken to prevent the mixture being thrown out of the flask, to which it is very liable, from the great weight of the precipitate preventing the regular escape of steam. The whole was filtered while hot, and the precipitate, which was of a dark-brown colour, after being well washed, dried, and heated to redness, weighed 111·5 grains.

(C.) The filtered fluid, which was extremely alkaline, was reduced to a convenient bulk by evaporation, and supersaturated with muriatic acid; carbonate of soda was now added in excess, which threw down carbonate of zinc, this was separated by the filter, and heated to redness; the oxide of zinc resulting weighed 14 grains, equal to 11·2 grains of zinc.

(D.) 100 grains of the precipitate (B) dissolved in dilute sulphuric acid left a small quantity of sulphate of lead, which, being separated, the solution was rendered very acid, and two polished cylinders of iron immersed in it, which precipitated the copper, this, when collected and dried, weighed rather more than 62·5 grains, which gives 70 grains of copper in the 111·5 of precipitate.

(E.) The solution from which the copper had been precipitated was boiled with nitric acid to peroxidize the iron, and nearly neutralized by carbonate of soda. Ammonia was now added in excess, and the oxide of iron thrown down by it separated by filtering.

(F.) The filtered ammoniacal solution was, with the addition of muriatic acid, evaporated to dryness, and the dry mass heated in a silver crucible to drive off the muriate of ammonia; after which the residue was dissolved in dilute muriatic acid, to which solution carbonate of soda was added in excess, and the precipitate resulting, after being heated to redness, weighed 22·5 grains, equal to 18 grains of zinc.

Thus, then, the products of the analysis are:

Copper at D	70·0
Zinc at C and F	29·2
	99·2
Loss	0·8
	100·0

Now had the mode of analysis under consideration been perfect, the precipitate (B) ought only to have weighed about 87·5 grains instead of 111·5, and that at (C), 36·5 instead of 14;—a difference much too great to be attributed to any error in the manipulation. Had the residue left by the potash been taken for oxide of copper, it would have given only 11·2 per cent. of zinc in the brass, which approaches pretty nearly the quantity assigned by some writers; and as the defect in the analysis probably arises from some mechanical cause, it is, therefore, likely that no two experiments would give the same results for an alloy of uniform composition.

In recommending another mode of analysis to the notice of chemists, I do not pledge myself that it is one which will give results of mathematical accuracy; but which nevertheless will be found sufficiently correct for any practical purposes, and may serve until some one more versed in analytic research than myself shall point out a better.

(A.) 70 grains of copper and 30 grains of zinc were dissolved in dilute nitric acid. The solution, with the addition of a little sulphuric acid, was evaporated to dryness, and redissolved in dilute sulphuric acid, adding the acid considerably in excess. A little sulphate of lead which remained was separated, but not weighed; it might arise from the zinc or the acid.

(B.) The solution being much diluted was introduced into a flask, and brought to the boiling point. Three polished cylinders of iron, one inch long each, were introduced, and the boiling continued until the solution became nearly colourless. A portion of the liquid was now tried for copper by a fresh piece of iron, but the surface remained perfectly free from it; and upon adding one drop of a solution of nitrate of copper, a precipitate took place, which shows that this test was sufficiently delicate.

(C.) The mixture was filtered while hot, and the copper well washed with very dilute sulphuric acid, and afterwards with boiling water; and being dried, was put in a crucible covered with charcoal powder, and melted, the button weighed 69·5 grains.

The filtered fluid was now proceeded with precisely in the same manner as at (E) in Experiment I. The oxide of zinc obtained weighed 36·75, equal to 29·4 of zinc. This gives

Copper	69·5
Zinc	29·4
	98·9
Loss	1·1
	100·0

The deficiency of half a grain of copper, and little more than half a grain of zinc, is not greater than what often occurs in analysis; and it is probable that even that quantity would be lessened by care in conducting the operations. In performing this analysis, it is necessary to drive off the whole of the nitric acid from the solution (A), and to have sufficient excess of sulphuric acid to prevent the copper from attaching itself too closely to the iron; in other respects, the excess of acid is not material. The boiling must be continued until such time as the solution becomes colourless, or till it ceases to give any indication of copper by any tests which may be applied. The solution should be filtered while hot, and the precipitate washed with very dilute sulphuric acid. If the copper can be sufficiently dried without the risk of oxidizing, the fusion of it may be dispensed with; but if it is thought necessary to melt it, the charcoal powder, after the fusion, should be carefully washed, as minute grains of copper are generally dispersed through it. The iron used for precipitating the copper should be as pure as possible, as frequently an insoluble black substance, which is probably a carburet of that metal, separates during the action of the acid: should that be the case to any considerable degree, so as to interfere with the results, it is highly probable that it would be got rid of by fusing the precipitate.

Boiling the solution during the precipitation of the copper has many advantages over simple immersion of the iron in a cold solution. By the former method, a complete precipitation is generally obtained in an hour; while, by the latter, the solution has held copper at the end of four days, and even when a complete precipitation has been obtained (which is not often the case), a portion of the iron deposits in the state both of protoxide and peroxide, which can only be got rid of, at the risk of dissolving a portion of copper with them; besides which, the copper frequently adheres with such obstinacy to the iron as to render the estimation of it impracticable.

As a menstruum from which to precipitate the copper, sulphuric acid possesses decided advantages over any which I have tried; if the alloy contains lead, it is left insoluble in the first instance; also the copper is not reacted upon when precipitated, as is the case when the nitric acid is employed; the progress of the operation too may be judged of from the colour of the solution. It is probable that the acetic acid might be employed, but I have not made trial of it.

Article III.

On the Geology of the Isle of Wight, &c.

By Prof. Sedgwick.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Trinity College, Cambridge, March 17, 1822.*

There appeared in the *Annals of Philosophy* for Sept. 1821, an article by one of your correspondents (Mr. G. B. Sowerby), on the Geology of Headen Hill, in the Isle of Wight. With some of his observations I entirely concur; and I appreciate as highly as he can do himself the importance of an intimate acquaintance with certain branches of natural history. Without such knowledge it must be impossible to ascertain the physical circumstances under which our newer strata have been deposited. To complete the zoological history of any one of these formations, many details are yet wanting. It is principally with the hope that my own observations may be in some measure subservient to this end, that I have ventured to request their insertion in your journal.

I must at the same time state, that some of Mr. Sowerby's views respecting the classification of the formations over the chalk differ widely from my own. An absence of several months from the University prevented his remarks from falling into my hands before December. A series of engagements, of which it is unnecessary here to speak, have prevented me from undertaking the task of replying to them until this moment.

The point on which your correspondent appears to differ most widely from myself is in the estimate he has formed respecting the merits of those who have preceded him. He seems hardly to have considered that the honours of inventive talent must chiefly be awarded to those who first point out the way to discovery, and from obscure and seemingly imperfect data are able to deduce important general conclusions. One thing, however, is most certain, that sarcastic animadversions, either on societies, or individuals, can never tend to promote the investigations of truth.

It is impossible that the praise of successful investigation should be withheld from Mr. Webster. His excellent and original communications respecting the formations in the Isle of Wight are in the hands of every English geologist, and prove both his diligence in the observation of facts, and his sagacity in drawing the right conclusions from them. No subsequent discoveries can possibly take away from this merit, even though they point out some objects of detail which had been, perhaps, either overlooked or misunderstood.

In the following notice I intend, first, to give some account of the fossils contained in the iron sand and the other beds immediately beneath the chalk. Secondly, to offer some remarks on the formations above the chalk. The observations on which the remarks are principally founded were made during two visits to the Isle of Wight; one in the early part of the year 1819; the other in the early part of last summer.

No paper in the *Annals of Philosophy* will be quoted, except the one before alluded to; it will, therefore, be only necessary to refer to the page in which any particular observation is contained. I have the honour to be, Sir,

Your most obedient servant,

A. Sedgwick.

I. *Observations on the Formations between the Portland Oolite and the Chalk.*

Since the publication of Mr. Webster's letters to Sir H. Englefield, the deposits between the Portland oolite and the chalk have generally been subdivided into three formations. (1.) *Iron sand,* a formation of ferruginous sand and sandstone. (2.) An argillaceous deposit (*Tetsworth clay* of Mr. Greenough). (3.) *Green sand,* a siliceous deposit, containing subordinate beds of limestone, &c.; generally characterized by the presence of a certain quantity of green earth. This classification is independent of all theory, and is only to be considered as a convenient mode of arranging a great many similar beds, the geological relations of which have been made out by actual observation. The three formations may together be considered as the representatives of the *quader-sandstein* of Werner, as they appear to be associated with rocks of the same age, and agree with it in some of their more minute characters. Many details must, however, be supplied before even this point can be completely established. From the whole analogy of the English coast, as well as from the best accounts with which we are acquainted, there can be no doubt but that some of the beds here described are widely extended in the north of France. In many parts of England, they preserve an extraordinary unity in their general relations and external character. On the whole, if we follow the great chalk escarpment towards the north-east, they gradually thin off, and sometimes, perhaps, entirely disappear. A few miles to the north of Flambro' Head, the chalk is seen in the cliff reposing on the clay of the vale of Pickering, without the intervention of any beds of sand or sandstone. With whatever certainty the great relations of the three formations above-mentioned may have been established, errors may arise in fixing points of detail from some of the following causes. (1.) The colouring principles of the upper and the lower sand are sometimes wanting. (2.) The green particles which characterize the upper sand sometimes abound so much in the lower as to alter its ordinary appearance. (3.) The red oxide of iron abounds so much in some masses of the *green sand* that, without care, they might be confounded with the ordinary beds of the *iron sand*. Another distinctive character, founded in the abundance of organized fossils in the *green sand* and their entire absence from many parts of the *iron sand*, might sometimes lead to erroneous conclusions. In the sandy parts of Bedfordshire, one may travel for miles together without seeing even a fragment of a fossil shell; yet some beds, which are subordinate to the same sand formation in the Isle of Wight, exhibit innumerable traces of beings which once possessed an organized structure. A good account of the fossils of the *iron sand* is undoubtedly a *desideratum*. Mr. Sowerby's work will, I hope, eventually supply this want. I shall consider myself fortunate if this brief notice should induce any one to visit those localities which best illustrate the zoological history of the formation.

All the upper beds of the *iron sand* in the Isle of Wight partake of the high inclination of the central chalk range (Mr. Webster's Letters, p. 122, &c. &c.). As the bearing of the coast between Freshwater Bay and Brook Point, is considerably inclined to the direction of the strata; they rise up in succession from the level of the beach, and are thus brought out under circumstances most favourable to a minute examination. The natural section between Culver Cliff and the middle of Sandown Bay affords similar facilities for observation. Between Brook Point and Sandown the strata are nearly horizontal, and many parts of the coast present perpendicular escarpments. The upper beds would, therefore, be inaccessible, were it not for the deep channels worn in the face of the cliff by the rivulets which descend from the central chalk range.

As the fossils which characterize the formation are not uniformly distributed through its mass, it may be proper briefly to enumerate some of the principal changes which are exhibited in the mineral composition of its subordinate beds.

(1.) Siliceous sand variously coloured by oxide of iron, the colours sometimes very splendid, and beautifully contrasted. With these beds are sometimes associated a kind of coarse siliceous grit (*carstone*) cemented by oxide of iron.

(2.) Fine white siliceous sand often passing into sandstone.

(3.) Beds containing a variable admixture of argillaceous matter. They often contain carbonate of lime, and a considerable quantity of green earth. The more siliceous varieties then assume the appearance of *green sand*. In all these varieties there are fossils, most frequently in the form of casts.

(4.) Beds of slate clay associated with pyritous wood coal. Some subordinate calcareous beds contain fossil shells, and innumerable bony fragments of a small fish.

(5.) Beds of impure shell limestone. They abound in some parts of Sandown Bay, and occur in almost every part of the cliff between Brook Point and Freshwater Bay. Along with them are several of those concretions described in the fifth volume of the Geological Transactions under the name of *Curl.*[*] Only such beds have been described as are found in the Isle of Wight, and may serve in some measure as guides to those who are examining its fossil history. It would be quite foreign to the objects of this paper to describe the beds of fuller's earth, plastic clay, yellow ochre, &c. which are found in other parts of England subordinate to this formation.

II. FOSSILS OF THE IRON SAND.

1. Obscure impressions of reeds and other vegetable bodies mixed with carbonaceous matter, and sometimes disposed in regular layers. They abound in the argillaceous beds, and are contaminated with the presence of much iron pyrites.

Carbonized wood is found in all parts of the formation. It sometimes makes an approach to the appearance of jet. More commonly the particles adhere so imperfectly that the specimens crumble between the fingers. Near Brook Point the masses of mineralized wood are seen at the time of low water scattered about the strand like the great beams of a timber yard. Mr. Webster[†] has given an accurate and graphical description of this portion of the coast. The changes undergone by these bodies are various and interesting. Traces of the original bark are by no means uncommon; and in one instance we found it marked with deep lozenge-formed indentations. Carbon and pyrites abound in almost all the specimens; and the several parts are often held firmly together by carbonate of lime which has insinuated itself into every portion of the mass, and partially displaced the woody fibre. When the lime is taken up by acids, there remains behind a friable skeleton of carbon. In other examples, the process of replacement is so complete, that the specimens may be regarded as true petrifactions. I thought this the more remarkable as all the specimens of similar origin which I have seen in the *iron sand* of Norfolk and other parts of England, are silicified. The existence of this vast quantity of fossil wood is the more interesting, as it points out an analogy between the *iron sand* and the *quader-sandstein* of Werner.[‡] Our information respecting the other fossils of that formation is too meager to afford us much assistance.

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[*] These concretions are found occasionally in the greywacke formations; they abound in many of our coal districts, and are by no means rare in certain portions of the Lias clay, the Oxford clay, and the Kimmeridge clay. Perhaps there is not one of our great argillaceous deposits in which traces of them may not be found. They most frequently appear in the form of distinct hemispheroidal concretions adhering either to the upper or lower surface of beds of ironstone, or impure argillaceous limestone, which traverse the masses of slate clay. Sometimes also they exhibit a conformable arrangement between two indurated beds, and seem intimately associated with layers of impure carbonate of lime which have a fibrous structure transverse to the strata. All these varieties are found in the Isle of Wight.

[†] Letters to Sir H. Englefield, p. 153.

[‡] See D'Aubuisson Traité de Géognosie, vol. ii. p. 223, &c.

2. Zoophytes.—(1.) We found between Freshwater Bay and Brook Point many cylindrical concretions, deeply tinged by the red oxide of iron. They present such well defined spheroidal terminations, that there can be no doubt of their being derived from some organized body, though they do not exhibit distinct traces of animal structure.

(2.) Stems of the body described by Mr. Webster under the name of the *tulip alcyonium*. The stellated transverse sections of the stem,* which are so common in blocks derived from the *green sand* formation, appear also in some beds of the *iron sand* near Red-cliff.

(3.) *A compound Madrepore.* The stellæ arise from a pedicle, and are grouped in a regular spheroidal form. Some of the larger specimens appear to be formed by the union of several distinct spheroids. They exist in the form of calcareous concretions, in a reef of indurated *iron sand*, which appears below the high water mark on the eastern side of Sandown Bay. It is a continuation of some of the inclined beds of Red-cliff, and its induration arises from the presence of a considerable quantity of carbonate of lime. I am the more particular in describing this locality, as it abounds in several fossil species which are in a state of perfect preservation. I unfortunately reached the spot when the tide was running in, and was only armed with a hammer and a small chisel. Any one who wishes to investigate the subject further, ought, after providing himself with more powerful weapons, to visit the reef before the time of low water.

(4.) An obscure coralline body. From the same locality. In its structure it has a general analogy with the bodies described by Parkinson, vol. ii. p. 137, 138.

3. Univalves.—(1.) *Vermicularia.* Sowerby, Min. Con. pl. 57. Several traces of this genus were observed in the Red-cliff reef above-mentioned. Along with them were some adhering *serpulæ*.

(2.) *Ammonites.* We found a single fragment of this genus in a bed of coarse greenish sand near Brook Point.

(3.) *Rostellaria.* *Casts*, sufficiently characteristic, to determine the genus, are found in the upper part of the cliff at Shanklin Chine.

(4.) A highly ornamented univalve, with raised longitudinal ribs, probably a rostellaria. In the same state with the preceding. We met with fragments of the shell near Brook Point.

(5.) *Vivipara* (?) A small species resembling that which abounds in the Purbeck marble. From the beds of shell limestone near the top of the *iron sand* formation in Freshwater Bay.

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* Geological Transactions, vol. ii. plate 29.

Casts, probably derived from the same shell, are found in some earthy beds between the last mentioned locality and Brook Point.

(6.) To this list may be added the *casts* of at least three or four species of univalves which abound in the upper part of the cliff near Shanklin.

Every one who has examined the localities of fossils must have remarked that in strata abounding in the oxide of iron, they are frequently stripped of their shelly covering. In soft argillaceous beds, even the colours of the original shell are sometimes preserved, but the specimens are generally friable, and often disfigured by compression. In other mineral masses, more especially such as contain a considerable proportion of carbonate of lime, the fossils which can be detached from the matrix are in a beautiful state of preservation. Several species which exist only as *casts* in the ferruginous cliffs of Shanklin, are finely preserved in Red-cliff reef. It is probable that perfect shells belonging to all those species of which we have now only the *casts*, may be found by any one who has time to examine the localities above-mentioned.

4. BIVALVES.—(1.) *Gryphæa sinuata.* Min. Con. t. 336.

(2.) *Ostrea.* At least three distinct species.

(*a.*) A palmated cockscomb oyster, associated with, and sometimes adhering to, the preceding species. They are found in regular beds which traverse the cliffs of Blackgang Chine and Shanklin Chine. Traces of the same beds appear to the west of Brook Point.

(*b.*) An undescribed species; much elongated; in general form somewhat resembling the *ostrea tenera*. Min. Con. pl. 252, p. 2, 3. This species abounds in some of the calcareous beds to the west of Brook Point.

(*c.*) A small flat oyster, associated with the preceding. Thin beds of this fossil traverse some of the argillaceous strata of Sandown Bay. The shelly matter is sometimes replaced by minute crystals of selenite which have originated in its destruction. The analogous facts exhibited by the Oxford clay and the Kimmeridge clay, are too well known to require any description. There are probably more species of this genus; but the specimens are generally too ruinous to show any good distinctive characters.

(3.) *Perna.*

(*a.*) A large thick quadrangular shell.

(*b.*) Resembling *perna aviculoides* (Min. Con. pl. 66), but the fragments too imperfect to determine the species. Both are found in Red-cliff reef. The *perna aviculoides* is a characteristic shell of the middle oolite formation (*coral rag*). A *perna* of rude trapezoidal form is also found in that formation near Weymouth, but is of an entirely different species from the quadrangular shell above mentioned.

(4.) *Trigonia.*

(*a.*) *Dædalea.* Min. Con. pl. 88.

(*b.*) *Alæformis.* Min. Con. pl. 215.

Both species are found in Red-cliff reef. Casts of the latter abound in the ferruginous beds of Shanklin Chine. It is very common in the *green sand* formation.

(5.) *Astarte excavata* (?) Min. Con. pl. 233. Red-cliff reef.

(6.) *Sphæra corrugata.* Min. Con. pl. 335. Red-cliff reef.

(7.) *Terebratula.* Too much imbedded to exhibit specific characters. Resembles *terebratula pectita.* It exists in great abundance, and in various states of preservation, in some beds near Shanklin Chine.

(8.) An exceedingly minute oval-shaped bivalve. It exists in immense abundance, often appearing like a thin farinaceous coating interposed between the laminæ of calcareous slate clay, which occur in the argillaceous beds of Sandown Bay, and various other parts of the formation.

(9.) A bivalve abounding in the thin beds of limestone which occur in so many parts of the formation. Casts of the same shell are common in some of the less indurated beds. We were in no instance able to obtain specimens with distinct specific characters.

(10.) To this list may be added the *casts* of four or five species which are in too imperfect a state to be ascertained. From the upper part of the cliff near Shanklin Chine.

5. VERTEBRAL BONES, FINS, &c. OF A SMALL FISH.—In great abundance in some impure calcareous beds, west of Brook Point. To these we may add the bones of a large cetaceous animal which were found, as I have been informed by Professor Buckland, in Sandown Bay.

The preceding list, however imperfect, will show that the *iron sand* is by no means destitute of fossil inhabitants. They are generally in a bad state of preservation, and little inviting to the collector. But on this very account, the few beds which exhibit them in a state sufficiently perfect for description are the more deserving of a minute examination.

III. TETSWORTH CLAY.

This bed, interposed between the *green sand* and the *iron sand*, is perfectly continuous in the Isle of Wight.* It is of comparatively little interest, inasmuch as it contains very few fossils. In the ruinous cliff formed by its western termination, we found fragments of a thin calcareous bed containing traces of a small species of *vivipara.* This fact, if confirmed by better specimens, would be interesting, because shells of the same genus abound in the Petworth marble, which is derived from the same formation.

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* See the plates accompanying the work of Sir H. Englefield on the Isle of Wight.

IV. GREEN SAND AND CHALK FORMATION.

The great geological phenomena presented by these two formations have been admirably detailed by Mr. Webster. Their fossil history has already received considerable illustration: much, however, remains to be done. The zoophytes (especially of the *green sand*) would afford excellent materials in the hands of a good naturalist, as the species are very numerous, and the greater part of them remain undescribed.

The beds of *green sand* in Freshwater Bay are in a state of imperfect aggregation which favours the extraction of the organic remains. The corresponding beds which appear near the eastern extremity of the island are in a state of much greater induration. They are there seen in contact with a variety of indurated marl which forms the basis of the chalk deposit. The line of junction is ill defined, as the green sand appears to pass into the beds which repose upon it, by gradations which are almost insensible. These ambiguous appearances are limited to the extent of a few feet, and can throw no difficulty in the way of a proper classification of the two formations. In some other parts of England, the case is far different. The argillaceous matter, which always forms a constituent of the lower chalk, prevails to such a degree that the beds gradually pass into a tenacious clay. On the other hand, the lower portions of this argillaceous marl become mixed with sand and sandstone, which seem to link them to the *green sand* formation. Hence arises an ambiguity.

The beds of argillaceous marl may be considered as forming a portion either of the *green sand*, or of the *chalk* series. Mr. Greenough, in his Geological Map, has chosen the former alternative. As far as my own observations go, the choice has, I think, been unfortunate. Perhaps the proper mode of avoiding all ambiguity would be, to give an appropriate name to all those varieties of chalk marl which are in the state of a tenacious clay, and to represent them by a peculiar colour. The strata in the neighbourhood of Cambridge afford an example of the arrangement we have been describing. The order of superposition is as follows:

(1.) Chalk with flints, forming an escarpment which ranges about 10 miles to the east of the town.

(2.) Indurated chalk marl, the upper portion approaching the true character of chalk, the lower portion becoming gradually so argillaceous, that it at length loses the appearance of a cretaceous rock.

(3.) Tenacious bluish clay (*galt*) separated from the preceding by a very thin bed, which is mixed with green sand, and contains a great many fossils. No denudation immediately about Cambridge shows any inferior formation; but on the confines of Bedfordshire, the *galt* is seen reposing on the *iron sand*.* This account might lead any one who is unacquainted with the fossils of the district to suppose that the thin bed of green sand is the representative of the *green sand* formation; and consequently, that the great bed of clay (*galt*) is identical with the *Tetsworth clay* before mentioned. The conclusion would not, however, bear the test of examination for the following reasons:

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* Geol. Trans. v. 114.

(1.) The fossils of the thin bed of green sand, amounting to about 20 species, do not belong to the suite of the *green sand formation;* but partake of characters common to the upper and lower beds associated with it.

(2.) The fossils of the clay (*galt*) form a suite nearly identical with those of the *Folkstone marl* which reposes on the *green sand formation.**

(3.) When the *galt* in the vicinity of Cambridge is perforated for the purpose of obtaining water, the first discharge forces up a considerable quantity of green sand—a fact which indicates the existence of the *green sand formation* below the *galt.*† All these facts combined with the intimate connexion between the chalk and the beds on which it reposes, lead to the conclusion that the *galt* of Cambridge is an argillaceous variety of chalk marl. Any system of classification which unites this great argillaceous deposit with the inferior *green sand formation,* assumes the existence of a relation respecting which the denudations of the country afford no evidence whatsoever.

Notwithstanding the intimate relations between the argillaceous marl and the superincumbent beds, it would not be expedient to represent it on a map by the ordinary colour of the chalk; for the colour would then cease to mark the boundary of a distinct escarpment. If these views be correct, the proper mode would be to represent the argillaceous varieties of chalk marl by a distinct colour; which would then mark the superficial extent of a flat region stretching out from the foot of a well-defined chalk escarpment. The mode recommended would moreover be in harmony with that which is adopted in the superficial delineation of the greater part of the English oolite series. This series (as is obvious from the enumeration in Mr. Greenough's Map) consists of three distinct oolitic deposits, each resting on a great bed of clay. The *Lias clay* and the *Oxford clay* have appropriate colours assigned to them. In conformity with the system, an appropriate colour ought also to be given to the Kimmeridge clay wherever it appears at the surface. The Portland oolite and the superincumbent Purbeck beds might then be conveniently represented by a single tint.

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* Geol. Trans. v. 57.

† The supplies of water obtained by *boring* have never been known to fail. This seems to prove that there is an impervious bed, probably of clay, immediately under the *green sand* on which the *galt* reposes. For it is hardly conceivable that a *large* formation of sand should be always so saturated with water as to be able to force up a column to the height of nearly 200 feet, wherever the upper bed is perforated.

Every one who is acquainted with the continental works on geology must have remarked the extraordinary meagerness of their details respecting most of the secondary rocks which are newer than the formations of the *Thüringerwald.* The English formations belonging to the same epoch are exhibited in a beautiful and perfect order, for which it is in vain to look in any other part of the world which has been yet examined. The great beds of clay, interposed between the several oolitic deposits, first enabled Mr. Smith to separate them from each other, and to trace their distinctive characters. All such points of detail, when once well established, become so many new terms of comparison, by which we may eventually be enabled to fix the relations between our own formations, and those in other parts of the world. An accurate delineation of all those argillaceous beds, which form so distinguishing a feature in English geology, is certainly an object of the first importance. Our geological maps, as far as they are constructed on that principle, not only point out the demarcation between mineral beds of separate characters, but acquaint us at once with the general aspect of the surface. The colours of the argillaceous beds representing the extent of low, marshy, featureless districts; while the colours of the several oolitic formations, and of the chalk, point out the limits of a succession of bold escarpments and prolonged natural terraces.

The preceding digression has not originated in any spirit of captious criticism; but in difficulties which have been experienced in classifying some of the beds which support the chalk; and in an earnest wish that the authors of our geological maps, who have supplied us with so many admirable details, may *leave nothing undone which can possibly be effected.*

V. ON THE FORMATIONS OF SAND AND PLASTIC CLAY, LONDON CLAY, &c.

The English formations which rest immediately upon the chalk belong to a distinct epoch in the natural history of the earth; for they are not co-extensive with, nor always conformable to, the beds by which they are supported, but rather resemble materials which have been mechanically drifted into the deep depressions or water-worn denudations of the older rocks. They are, therefore, generally limited to the extent of certain pre-existing inequalities in the surface of the globe.

Deposits originating in the way we have described must necessarily be of variable thickness, and liable to every possible modification from the action of mere local causes. Any useful classification of their component beds would, perhaps, never have been effected, had not the organic remains preserved in them exhibited an extraordinary uniformity of character and arrangement. An accurate examination of these spoils has, therefore, supplied us with the means of establishing analogies between phenomena which otherwise must have appeared entirely unconnected. As the physical characters of any strata become more liable to variation, all the accompanying circumstances are of proportionally greater importance. Hence, arrangements of some of our *formations* have been made exclusively from zoological considerations. It must, however, be obvious, that classifications of this kind would be of no use to the geologist, unless accompanied with direct observations on the superposition of the mineral masses in which the organic remains are entombed. The existence of a given suite of fossils in any rock proves nothing respecting its age, unless we can show that the same suite is associated with other strata of known relations; and even then, our conclusions on the subject are only founded on analogy. No one ought, therefore, to generalize on such grounds as these before his observations have been widely extended. He may then become acquainted with so many corresponding facts; that phenomena, which at first presented nothing but obscure analogies, may at last become as conclusive in establishing any geological relation as the most direct evidence.

The truth of this statement is so obvious that it would have been unnecessary to bring it forward, had it not been overlooked by some of those who object to the present arrangement of our *tertiary rocks.*

The lowest, and at the same time the most widely extended deposits in the *chalk basins* of London and the Isle of Wight, are generally divided into the two following *formations:*

1. *Sand and Plastic Clay.* Composed of a great many beds of sand, in some places white and pure; in others impure and tinged with almost every possible shade of colour. Subordinate to which are beds of fine potter's clay, impure argillaceous beds sometimes containing calcareous concretions, beds of rolled flints, thin beds of impure wood coal, &c. The beds of sand and pebbles appear to have originated in a state of things ill suited to the preservation of organized beings: accordingly, with some remarkable exceptions, we find few of their remains in this formation.

2. *London Clay.* A *formation* of tenacious clay, often abounding in *septaria;* sometimes containing thin beds of argillaceous limestone; containing also, more rarely, beds of sand and calcareous sandstone. Organic remains are dispersed in most extraordinary profusion through almost every part of this widely extended *formation.*

The separation of the two *formations* above-mentioned is not marked out by any extraordinary natural epoch; but is merely assumed as a convenient classification, founded on constant geological relations; on a decided difference in the composition of the constituent beds; and a still more decided difference in their zoological phenomena. It was first pointed out by Smith (Farey's Survey of Derbyshire, p. 110, 111). Parkinson afterwards furnished us with many interesting details, and established the complete identity of the great argillaceous deposits of the London *basin*, and of the Hampshire coast. (Geol. Trans. vol. i. p. 336). But it was reserved for Webster to describe a still more important series of facts which went far to complete the natural history of the two formations. The propriety of this classification has been further confirmed by elaborate sections taken by Buckland and Conybeare from certain parts of the London *basin*.* (Geol. Trans. iv. 277.) We have been thus particular in enumerating authorities, because they all tend to establish a conclusion which it has lately been attempted to invalidate. On referring to the specific objections urged against the classification we have been describing, it will be seen that they are all founded on the examination of a single natural section. (*Annals of Philosophy*, p. 217, 218.) The statement contains, therefore, in a great measure, its own refutation. No man living can on such grounds judge of the propriety of any geological arrangement; still less is he entitled to assert that every thing is in favour of his own opinion. It requires repeated comparisons before we can form a correct estimate of the extent and relative importance of any system of beds; and without that knowledge we are not in possession even of the elements of a good arrangement.

The extraordinary vertical beds which appear in Alum Bay, and are continued in the same position on the north side of the chalk range to the eastern extremity of the Isle of Wight, are separated by Mr. Webster into two *formations*. (Geol. Trans. vol. ii. pl. 11, &c.)

(1.) *Sand and Plastic Clay*. Extending from the chalk to the bed of cemented pebbles marked (*z*.)

(2.) *London Clay*. Represented in this instance by a bed (B) about 250 feet thick.

Against this system it is urged (*Annals*, p. 217), that there is a bed of *London clay* marked (*d*) in the same section, *below* the greater part of the beds of *sand and plastic clay*; that there is as much *apparent* continuity in the bed (*d*), as in the one marked (B).

To this we reply, that there is no *apparent* continuity in either of the beds. Nothing is placed before the eyes except an irregular surface laid bare by a section transverse to the range of the formations above the chalk. But we do know that a great argillaceous deposit, resting on the *sand and plastic clay formation,* is almost co-extensive with the *basins* of London and of the Isle of Wight; and that its general relations, and the fossils contained in it, are identical with those of the stratum (B) in the section of Alum Bay. On the other hand, we do not know, by the evidence of other sections, that the stratum (*d*) is continuous. Indeed there is all the proof which can be afforded by negative evidence that it has no rank but that of an accidental or subordinate bed.

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* The pits at Catsgrove Hill, near Reading, are noticed by Dr. Woodward, and a series of specimens collected by him from that locality, are still lodged in the cabinets of the University of Gambridge. His account of the section, as it existed more than 100 years since, is as follows: (Cat. of Fossils, vol. ii. p. 41.)

"The uppermost stratum is of gravel, about two feet thick. Thin clay of various colours, purple, blue, red, liver-colour, 33 feet. Next the sand, with the oyster shells, composed of grains, greenish, black, and white, one foot thick. Under this a dusky clay, with some oyster shells in it, but very tender and rotten, a foot thick. Underneath, chalk, in which the workmen have sunk 20 feet without finding the bottom," &c. He afterwards adds, that the bed of sand with oysters had been traced for two miles on every side of Reading.

It is further stated (*Annals,* p. 218) that the fossils of the lower argillaceous bed (*d*) are identical with those of the *London clay*. This assertion, taken in its utmost extent, only proves that the sea, during the age of the *sand and plastic clay formation,* was sufficiently tranquil to allow the propagation of certain species of *molluscæ*. Nor is it extraordinary that the progeny of these animals should be found in still greater abundance in newer argillaceous beds, deposited under circumstances more favourable to the existence of organized beings. But after all, is the assertion correctly true? The argillaceous bed (*d*) was examined by Mr. Henslow and myself during our first visit to the Isle of Wight in the year 1819.

The fossils of the stratum were principally confined to the *septaria*. Among the specimens brought away on that occasion, I find the following:

(1.) Fragments of an oyster nearly resembling *ostrea pulchra.* (Min. Con. pl. 279.)

(2.) A *mya* and *pinna,* both probably *London clay* fossils; because, if I mistake not, the same species are found in the rocks of Bognor.

(3.) Casts of two species of bivalves, and of one univalve.

It is worthy of remark, that not one out of this suite is figured in the *Fossilia Hantoniensia* of Brander. On the other hand, thousands of fragments of the well-known *London clay* fossils are dispersed through almost every part of the upper argillaceous bed (B); agreeing (as was observed by Mr. Webster) in their state of preservation as well as in their specific characters with the organic remains of the Hampshire coast.

Two other facts are brought forward (*Annals,* p. 218) to prove that the beds we have been considering ought not to be separated into two *formations,* viz. the existence of *septaria* in the stratum (*d*), and the existence of decomposing rolled flints both above and below the argillaceous bed (B). We may briefly observe; first, that *septaria* prove nothing respecting the age of the beds in which they are contained; because they are found in all the argillaceous deposits associated with our secondary rocks; and secondly, that rolled flints prove nothing except the mechanical origin of the banks of sand in which they are contained, and may be found in any stratum which is newer than the chalk.

Had we possessed no information respecting the beds immediately above the chalk, except that which is derived from the Isle of Wight, no one would, perhaps, have thought it necessary to separate them into two *formations*. So much may safely be conceded to the objections we have been considering. We must at the same time remark, that the section in Alum Bay can hardly convey any correct notion respecting the classification of the component beds, because the regular order of deposition has been interrupted by a catastrophe which hurled all the strata into a position entirely different from that which they once occupied. Under such circumstances, it would be most unphilosophical to hasten to a general conclusion before other localities have been examined, in which the successive deposits have met with no interruption, and in which all the accompanying phenomena are exhibited in more perfect order. The Hampshire coast from Studland Bay to the eastern termination of Hordwell cliff, offers the best possible commentary on the *formations* of Alum Bay; for in that part of the *basin*, the lower beds have not been displaced since their first deposition, and are laid bare by a succession of good sections. The *sand and plastic clay* formation occupies every portion of the cliff between Studland Bay and Christ Church Head. In following the coast in that direction, the beds are found to have a slight inclination in a direction about ESE. At the termination of the cliff of diluvium on the east side of the Christ Church river, these beds reappear with the same inclination which gradually carries them under the *London clay*. The line of demarcation is perfectly well defined, and the *London clay* then occupies the whole cliff, and exhibits a succession of beds of very great thickness. By the continued inclination towards the imaginary centre of the *basin*, they are successively brought down to the beach, and at length disappear (nearly opposite the village of Barton) under *formations*, which are evidently contemporaneous with the horizontal beds of Headen Hill, and will be mentioned in their proper place.

The portion of the *sand and plastic clay formation*, south of Poole harbour, may be examined in any of the great pits which have been opened in it. The following section, taken from one of the clay-pits to the north-west of Corfe Castle, may serve as an example. The beds are counted from the top.

(1.)	Yellow sand with ferruginous concretions. . .	20 or 30 feet.
(2.)	A thin bed of ferruginous grit resting on impure wood coal	2
(3.)	Fine potter's clay	8
(4.)	Ferruginous grit, and very impure pyritous wood-coal	2
(5.)	Good clay	5
(6.)	Dark impure clay	3
(7.)	Fine potter's clay	8
(8.)	Impure sand and clay of unknown thickness.	

The sandy cliffs, west of Christ Church Head, are traversed by irregular subordinate argillaceous beds, some of which contain calcareous concretions. The presence of organic remains may be expected in such cases, though my very limited observations did not enable me to detect them. It is impossible to give any description of the *London clay* without entering on details which are incompatible with the objects of this notice.

The natural section exhibited between Handfast Point and the western termination of Hordwell Cliff, not only affords a satisfactory confirmation of the classification which has been adopted; but by a slight imaginary prolongation of the beds may be linked to another natural section between Colwell Bay and the Needles of the Isle of Wight. These general views enable us to connect the great depositions on both sides of the Solent, and to fix the relations of the vertical beds of Alum Bay by evidence not short of demonstration.

VI. BEDS BETWEEN THE LONDON CLAY AND THE LOWER FRESHWATER FORMATION.

The vertical beds of Alum Bay are succeeded by a formation principally composed of siliceous sand; the lower portion of which is considerably inclined, while the upper portion is nearly horizontal. It should seem, therefore, that this formation belongs to the epoch of the great catastrophe which tilted the central chalk range into its present unnatural position. The section in Whitecliff Bay confirms this hypothesis. All its more important features are beautifully delineated in the work of Sir H. Englefield on the Isle of Wight; and the accompanying descriptions by Mr. Webster prove that its component parts are contemporaneous, and probably continuous, with the formations of Alum Bay. The two localities, however, differ from each other in some points which are not unimportant. The *lower freshwater* beds rest immediately upon the most northern *vertical* bed of Whitecliff Bay, and descend so rapidly to the level of the beach that it is impossible to ascertain the nature of the strata by which they are afterwards supported. Nor is this the only distinguishing circumstance. The last vertical bed bears little resemblance to the *London clay* (D) of Alum Bay; for it is principally composed of siliceous sand, and contains a distinct suite of fossils. I have no specimens of these fossils now before me; but from memoranda made upon the spot, they appear chiefly to consist of the following genera:

(1.) *Ostrea*, with a convex and deeply striated valve.
(2.) *Venus*.
(3.) *Nucula*.
(4.) *Murex*, two species.
(5.) *Rostellaria rimosa*.
(6.) *Cerithium*, one or two species.
(7.) *Ancilla subulata*, &c.

With the exception of the undescribed oyster, all these species are found in exactly the same state of preservation, in the *upper marine formation* of Colwell Bay. From all these facts, which at first appeared sufficiently perplexing, we concluded; first, that the *London clay* had never extended to Whitecliff Bay, or, at least, had thinned off to such a degree as to be quite insignificant; and, secondly, that the most northern vertical bed of Whitecliff Bay was part of a formation of siliceous sand, &c. between the *London clay* and the *lower freshwater* beds. An examination of the Hampshire coast completely confirmed us in this opinion. It has already been stated that the *London clay* terminates in the cliff nearly opposite the village of Barton. Beds of sand of a light-brown colour there first make their appearance at the top of the cliff, and follow the dip of the inferior clay. They are succeeded by other beds of sand, containing two well defined layers of coaly matter which may be traced in the escarpment for a considerable distance. On the last mentioned sandy strata rests a bed, five or six feet thick, which contains a considerable portion of argillaceous matter. The whole system of these beds above the *London clay* is more than 40 feet thick. The lower sandy strata contain a few marine shells, among which we remarked some very large *cerithia;* but the highest beds abound in many species which (in addition to a few *London clay* fossils) form a suite, absolutely identical with that which characterizes the most northern *vertical* bed of Whitecliff Bay.* At the eastern end of this portion of Barton Cliff commences a freshwater formation, resting on the beds last described, and dipping like its associates about ESE. As nearly all the fossils I collected from this part of the coast were unfortunately lost, I am compelled to rely almost exclusively on memoranda made by my friend Mr. Whewell, who assisted me in collecting many of the facts detailed in this paper. The notes were, however, taken on the spot; and are, therefore, we hope, sufficiently accurate to establish the *general* facts for which we are contending. It appears from what has been stated; first, that a regular *marine* formation principally composed of siliceous sand, separates the *London clay* from the *freshwater beds* of the Hampshire coast; secondly, that the same formation is probably continued without interruption from Alum Bay to Whitecliff Bay; and, thirdly, that the disturbing force which upset the mid region of the Isle of Wight, acted after a part of this formation had been deposited. A deposit preserving the same characters in places which are so remote from each other, may probably have extended over the greater part of the Isle of Wight *basin.*

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* It is too much to expect the same phenomena in all the minute parts of a tertiary deposit; otherwise one might look for a similar suite in the bed (E) which separates the white sand from the *lower freshwater formation* of Headen Hill. (Geol. Trans. vol. ii. pl. 11.) We last summer only examined this bed in a single point, where it had been exposed by a *land slip* immediately above the great sand pits. It there contained very little argillaceous matter, but was composed of various coloured sands, which appeared to have been rudely blended together in agitated water. We found no fossils in it.

The siliceous beds which in the neighbourhood of Paris are often found between the *calcaire grossier* and the *lower freshwater formation,* are strikingly analogous to the beds we have been describing. Whether the *Bagshot sand* which rests immediately on the *London clay* can be referred to the same epoch may admit of doubt, because the time of its deposition is not, I believe, limited by any known succession of newer beds. The proper data for solving the question will probably be given in the next volume of the Geological Transactions.

VII. HORIZONTAL BEDS IN THE NORTH OF THE ISLE OF WIGHT, &c.

Since the publication of Mr. Webster's letters to Sir H. Englefield, it has been universally admitted that all the northern region of the Isle of Wight is composed of nearly horizontal beds, of later origin than any of those which have been enumerated. As the separation of these beds into three distinct formations is founded simply on zoological considerations, without any primary reference to the nature of their component parts; the propriety of such a separation can only be established by a rigid determination of the fossil species contained in them. With the completion of this task Mr. Webster has been for some time employed; especially since the objection to his arrangement, which appeared in the *Annals of Philosophy* for last September. Every thing may be expected from the zeal and talents of one to whom we are already so much indebted. Those facts, connected with this subject, which have fallen under my own observation, will be given without any details, except such as are absolutely necessary to make them understood. The classification published by Mr. Webster will be adopted without any reserve.

1. *Lower Freshwater Formation.* During a visit to the Isle of Wight, made by Mr. Henslow and myself in the year 1819, we verified almost all Mr. Webster's observations on the portion of this formation which extends from Headen Hill to Colwell Bay. In the northern part of Totland Bay, we found alternating with the indurated *lower freshwater* marl, several thin beds of clay, one of which contained many specimens of a small shell we considered a *cerithium.* Not only from its associates, but also from its specific characters, we are now convinced that it is a freshwater shell. According to the modern nomenclature, it must, therefore, be called a *potamides.* In some of the upper beds of the formation (more especially in Colwell Bay, near the place where they descend to the beach and disappear) we found undoubted proofs of the mixture of marine and freshwater species, not only in the argillaceous marls, but in the masses of freshwater rock. A single specimen struck off from one of these masses contains examples of the following genera:

(1.) *Ostrea.*
(2.) *Venus.*
(3.) *Cerithium.*
(4.) *Planorbis.*
(5.) *Lymnaea.*

The lower portion of the cliff between Gurnet Point and East Cowes presents many examples of the mixture or alternation of marine and freshwater genera, which cannot be accounted for merely by the degradation of the upper part of the cliff. This fact, and the probable reasons of it, are both stated by Mr. Webster (Geol. Trans. ii. 213). In every portion of the coast where there is any escarpment between Whitecliff Bay and Bembridge Ledge, and also between the mouth of Brading Harbour and Priory Park, we found well defined beds of the *lower freshwater formation.* We have already remarked their junction with the vertical beds of Whitecliff Bay. Immediately to the north of this junction, and from thence to Bembridge Ledge, many of these beds lose the character of indurated calcareous marl, and pass into a variety of hard shell limestone. In this state they are quarried to a considerable extent, and the larger blocks are cut down by a saw into forms which are suited for exportation. These rocks do not, as far as we observed, contain any marine spoils; but they exhibit innumerable traces of the common freshwater fossils; viz. paludina, planorbis, and lymnæa. Nor are they, as in some other places, at all confounded with the *marine marl* which rests upon them. A thin oyster bed of the *upper marine* marl may be traced in many parts of the bay where there is a clean escarpment, in almost immediate contact with the inferior rock. The beds just described have been referred by mistake to the *upper freshwater formation.* (Geol. Trans. ii. 228.) It would hardly have been necessary to notice this oversight had it not been copied by those who have described this part of the island without any personal examination of it. Either a dislocation, or at least a considerable flexure, of the freshwater strata, takes place at the entrance of Brading harbour; for on the south side of the harbour they dip to the north, but on the north side of it they dip at a more considerable angle in an opposite direction. The remaining part of the cliff as far as Priory Park presented a repetition of the same phenomena, viz. the *lower freshwater* rock surmounted by the argillaceous marl of the *upper marine formation.* The demarcation was, however, no longer well defined, but showed a mixture or alternation which probably originated in a gradual passage of one formation into the other. An examination of this part of the Island convinced us that Mr. Webster had correctly classed the calcareous beds near Ride with the *lower freshwater formation.* We had before adopted a contrary opinion. In addition to the difficulty of accounting for the appearance of any portion of the *upper freshwater* rock in the cliff between Gurnet Point and Ride; we may observe, that the mineralogical character of the calcareous beds in that part of the coast almost compels us to unite them with the similar beds near Brading harbour and Whitecliff Bay.

It would be improper not to mention in this place the appearance of the lower freshwater beds between Yarmouth and Gurnes Point. In that part of the coast they generally lose the appearance of an indurated calcareous marl, and pass into masses of stiff clay or argillaceous marl, which are not unfrequently of a bright green colour. Many of these green beds preserve their continuity for a great extent; and their order of superposition is beautifully traced out wherever the lower part of the cliff is not masked by the rubbish which is perpetually descending from the upper argillaceous beds. It seems impossible to avoid arranging them with some of the rocks we have been describing: Perhaps they may be the representatives of the highest part of the *lower freshwater formation* which is so frequently associated with beds of argillaceous marl. In this instance we may consider the calcareous portion of the formation to have been almost excluded by its associate. These beds of argillaceous marl are no where more perfectly exposed than in a part of the coast between Yarmouth and Hampstead Cliff, which is covered at high water; for in consequence of a very unusual angle of inclination towards the south, they are brought out one from under another in a long succession. The whole formation is subdivided by almost innumerable layers of fossil shells which follow the planes of stratification. The beautiful preservation even of the minutest characters both of the bivalves and of the univalves, and still more, their arrangement in distinct families, afford a proof not short of demonstration, that the whole system has originated in a tranquil deposition. Unfortunately the specimens are so friable that they generally fall to pieces when they are extracted from their matrix. Among these fossils we remarked an abundance of some of the following genera:

(1.) *Paludina.*
(2.) *Potamides.*
(3.) *Melania,* more than one species.
(4.) *Cyclas,* two species.
(5.) *Unio* (?) generally in the form of large casts in which the *nacre* is beautifully preserved.
(6.) *Planorbis,* more than one species.
(7.) *Lymnæa,* more than one species.

The two last were not so abundant as the others. There were also in the partings of some of the beds traces of vegetable impressions. From the green marl beds of Thorness Bay, we obtained very fine impressions of a kind of large flag. Adding what has been stated to the details already published by Mr. Webster, we may conclude, that in every part of the north coast of the Isle of Wight, from Alum Bay to Whitecliff Bay, where there is a good denudation, the *lower freshwater formation* may be traced, generally near the base of the escarpment, and surmounted by ruinous masses of argillaceous marl. In sinking a well for the use of the barracks near Newport, they are said to have brought up from the depth of 265 feet some specimens of green marl containing fossils exactly resembling some of those which abound at the foot of Hampstead Cliff. I am in possession of one of these specimens, which, if its locality be correctly given, almost proves that the *lower freshwater formation* exists in some places near Newport about 260 feet below the surface.

An examination of the old cabinets of Dr. Woodward had long since led me to expect that very important traces of the freshwater formations would be found on the Hampshire coast; but I had never, before last July, an opportunity of verifying the conjecture. After an excursion made by Mr. Whewell and myself to the neighbourhood of Christchurch, we returned by the Barton and Hordwell cliffs. This enabled us to observe the first appearance of the *London clay*, its termination in the cliff between the villages of Barton and Hordwell, and the beds of marine sand which succeeded, as we have already mentioned. The existence of a freshwater formation, extending nearly through the whole of Hordwell Cliff, was certainly more than we had ventured to anticipate. Any minute details which we might offer respecting this part of the coast would be received with distrust, as we lost nearly all the specimens we collected from it; and the attempt would be unnecessary, as Mr. Webster has subsequently undertaken the description of it. We shall, therefore, content ourselves with stating, that the formation which succeeds the marine sand above-mentioned is composed of various coloured beds of sand, loam, carbonaceous clay, argillaceous and calcareous marl seldom in a state of induration, several carbonaceous beds, some of which are three or four feet thick, &c. &c. All these beds are succeeded by some thick beds of bluish argillaceous marl, which are prolonged to the east, and at length carried, by the natural dip of the strata, under a cliff of diluvian gravel. Among the characteristic fossils may be enumerated the following:

(1.) *Planorbis.*

(2.) *Lymnæa.*

(3.) *Paludina.*

(4.) A small bivalve which resembles *mya;* it exists in immense abundance in the freshwater formations of the Isle of Wight.

(5.) *Melanopsis.*

(6.) *Cyclas.*

(7.) *Unio.*

(8.) *Potamides*, &c. &c.

We found, however, several *cerithia*, and some other shells which we at the time considered of marine origin. From this circumstance we were disposed to regard the great deposit of Hordwell Cliff as the representative both of the *lower freshwater* and *upper marine* formations of Mr. Webster. No part of the coast appears to offer any trace of the *upper freshwater* deposit.

2. *Upper Marine Formation.* By this we understand all the beds of argillaceous marl, sand, &c. which in Headen Hill are interposed between the two freshwater formations. (Geol. Trans. vol. ii. pl. 11.) They occupy a very wide superficial extent in the north of the island; yet there are few places in which their natural history can be studied with much advantage, if we except the cliff between Headen Hill and Colwell Bay, the argillaceous beds of Hampstead Hill, and the upper part of the escarpment in Whitecliff Bay. Mr. Webster derived from the first of these localities all the facts which were connected with the zoological history of the formation. In our examination of this deposit, we were convinced that its true limits could not be perfectly ascertained without a more rigid determination of the genera of its fossil inhabitants than had yet been attempted; we did not, however, by any means, arrive at the conclusion stated by Mr. G. Sowerby (*Annals of Philosophy*, p. 219), viz. "that if we depend upon fossil as the principal means of identifying strata, we shall see great reason to believe that there does not exist any marine formation between the two freshwater ones." As the determination of this question is of considerable importance, we think it proper briefly to state the grounds of our opinion.

(1.) In general, the lower calcareous beds appear to have been tranquilly deposited in freshwater. But if we ascend to the argillaceous marl which rests immediately upon them, we not only find a complete change in the physical circumstances of the deposit, but a new suite of organic remains, some of which are of marine origin, others of a doubtful character, and a few are identical with those in the lower beds. All this seems to indicate a marine inundation. Without some such interruption, it is not possible to conceive how a single marine shell should have found its way into a freshwater deposit. On the contrary, it is perfectly conceivable that any quantity of freshwater shells should have been drifted down into a marine deposit, and become so much mixed with it as to have altered its whole character.

(2.) There is much direct evidence to prove that the marine inundation lasted for a considerable time. The oyster bed above the freshwater rock in Whitecliff Bay has been already mentioned. Two oyster beds may be traced in some parts of Headen Hill; and single oysters are dispersed in many other portions of the formation.

Mr. Webster pointed out a great bank of oysters in Colwell Bay, many of which have their valves united, and are locked together in the way in which they usually live. This bank is several feet thick, and the species could hardly have existed in their present state had they not been propagated on the very spot where they are found. Again, in the great argillaceous deposit between Headen Hill and Colwell Bay, and also in the cliff near Hampstead, other marine shells are found in a beautiful state of preservation. The ruinous condition of the cliff precludes the possibility of determining their exact arrangement. They appear, however, chiefly to abound in the lower part of the deposit. Without pretending to give a complete list even of the specimens which we collected, it may be proper to enumerate the following genera:

(1.) *Murex,* at least two species abound in Colwell Bay.
(2.) *Buccinum.*
(3.) *Ancilla subulata,* by no means a rare shell in Colwell Bay.
(4.) *Voluta,* resembling *voluta spinosa.*
(5.) *Rostellaria rimosa.*

The last two mentioned species are certainly rare. There are two or three other *Londay clay* fossils in the marine beds of Colwell Bay: e. g. the *murex effossus* and *murex innexus* of Brander (Foss. Hant. Nos. 28, 30), and fragments of a species of *fusus.*

(6.) *Natica.*
(7.) *Venus.*
(8.) *Nucula.*
(9.) *Corbula,* a small species is very common in Hampstead Cliff.
(10.) Two small bivalves (of the genus *corbis?*) abound in the upper part of Hampstead Cliff. One of them is also common in Colwell Bay.
(11.) *Mytilus.*

This list, however imperfect, is quite enough for our present purpose. Along with the preceding species are some which are, perhaps, of an ambiguous origin; and others which are undoubted freshwater shells. A few of the latter specimens were probably drifted into the beds at the time of their formation; but the greater number may have descended into their present situation by the degradation of the superior strata. As we ascend to the higher parts of the formation, we find innumerable examples of certain genera which do not exist in this climate, but have been discovered in various parts of the world among the inhabitants of freshwater. The *cyclas* and *potamides* are most abundant, and with them are generally associated the *melanopsis* and *nerita,* &c. There are two species of *neritæ,* one of which nearly resembles the *nerita fluviatilis.* As, however, the beds abounding in these genera also contain a few marine shells, may they not have been deposited in the brackish waters of an estuary, or in a basin which was still partially affected by a marine inundation? In some beds at the top of the formation, which are exposed by a landslip in the middle of Totland Bay, all traces of a marine origin have disappeared. The phenomena exhibited by the section are as follows, beginning from the bottom:

(1.) Bluish clay containing innumerable fossils of the genus *potamides,* &c. Whole thickness not exposed.
(2.) Yellowish sand with the *lymnæa, paludina,* and *planorbis* .... 3 feet.
(3.) Carbonaceous bed 1
(4.) Yellowish sand with many specimens of *potamides, melanopsis,* and *cyclas* 2½
(5.) A thin coaly bed.
(6.) Sandy beds without shells .... 2 or 3

Immediately over these was the *upper freshwater* calcareous rock forming a bold escarpment.

In the Hampstead Cliff, the argillaceous marl beds of this formation are considerably more than 100 feet thick. They contain a great many fossils in a beautiful state of preservation, among which are five or six species we did not find in any other part of the island. In the upper part of the cliff, not far from the capping of diluvian gravel, there are some thin beds entirely composed of four or five species of shells, which have been driven pell-mell together, and now adhere to each other like masses of Suffolk *crag*. Even at this great elevation, we found a thin bed filled with a small shell of the genus *corbula,* resembling that which is figured by Sowerby, t. 209, f. 4.

From all that has been stated, we conclude, that the whole *formation* originated in an interruption to the deposition of the beds of calcareous marl occasioned by a marine inundation; that the lower part of the formation may be considered of decidedly marine origin; that some of the intermediate beds may have been formed during a partial or interrupted communication with the sea; and, lastly, that some of the upper beds were deposited in a part of the basin from which the sea was entirely excluded. In a single instance, we found a fragment of a small bone, and a beautiful vertebral joint of a fish in one of the marine beds of Totland Bay. This fact is worth recording, as it had not been remarked before; but it throws no light upon the present question. On the whole, the name of *upper marine formation* may, perhaps, be conveniently applied to the whole system of beds between the *upper and lower freshwater formations;* though the extended labours of naturalists have proved, since the publication of Mr. Webster's paper, that several of its fossil inhabitants belong to genera which are now only known to exist in *freshwater.*

There are two other observations in the *Annals of Philosophy,* p. 220, which we shall briefly notice; first, "That the Woolwich beds may be contemporaneous with this upper marine formation, for many of the shells contained in it are species of freshwater genera;" and, secondly, "That the *crag* on the coasts of Suffolk and Essex bears evident marks of identity with alluvium." In regard to the first of these observations, without stopping to notice the manner in which it is brought forward, we reply, that the order of superposition cannot be determined against the direct evidence of sections. If it can be shown that the section given in the Geological Transactions * is erroneous, the argument may then be listened to, but not till then. Again, freshwater shells may be expected to appear occasionally in any bed of a tertiary deposit. They exist in great abundance in some parts of the *plastic clay formation*,† between the *calcaire grossier* and the chalk of the Paris basin. Their existence in the *sand and plastic* clay of the London basin only establishes a new analogy between the contemporaneous *formations* of the two countries.

The observation on the Suffolk *crag* is given with too much confidence. Some of the masses of broken shells might have originated in the diluvian action which formed our great beds of gravel. But in other places the shells make an approach to a more regular arrangement, and are often associated with thick beds of siliceous sand. The coast of Essex may, perhaps, give a clew to the true relations of the deposit. At all events we think that even the imperfect denudations of the Suffolk coast prove that the *crag* is superior to the *London clay*. In the absence of more perfect details, I will transcribe two important sections which I obtained from my lamented friend Dr. E. D. Clarke. They are made out from a register of the borings of the strata undertaken last year in the hopes of finding a spring of freshwater for the town of Harwich.‡

First Section in the Town of Harwich.

	Feet.	In.
(1.) Soil	3	0
(2.) Sand, a strong spring of salt water.	17	0
(3.) Blue clay	20	0
(4.) Shingle and gravel	20	0
(5.) Red coarse sand	7	0
(6.) Coarse gravel	4	0
(7.) Coarse dark sand	6	0
(8.) Clay green and red	1	2
(9.) Green clay	1	10
(10.) Chalk	28	0
(11.) Chalk mixed with fine sand	0	9
(12.) Chalk, grey from the mixture of dark sand; several flints and pieces of septaria	72	0
(13.) Pure carbonate of lime	176	0

Making in all 276 feet of chalk when the work was abandoned.

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* Vol. iv. pl. 13.

† I have been informed by my friend Mr. Underwood, of Paris, that many of the French naturalists now consider the *plastic clay* as the *lowest freshwater* deposit of the Paris basin.

‡ Before these borings were undertaken, no chalk had ever been reached in the neighbourhood.

Second Section, commencing 198 *Feet South of the preceding, and Eight Feet above the High Water Mark.*

	Feet.
(1.) Soil	4
(2.) Sand containing salt springs.	12
(3.) Blue clay, containing selenite, &c.	74
(4.) Gravel, with vegetable matter	Unknown

In the neighbourhood of Harwich, the *crag* is associated with No. 2. No. 3 is probably the representative of the *London clay*. Nos. 4, 5, 6, 7, 8, 9, will then represent the *sand and plastic clay formation*.

If these views be correct, Mr. Smith's arrangement of the *crag* must be considered quite untenable.

3. *Upper Freshwater Formation.* We have now arrived at the last of the *formations* discovered by Mr. Webster—a system of calcareous beds more than 100 feet thick, which appear to have been deposited in a freshwater basin to which the sea had no longer any access; for we find in it no marine shells, and very few of those species for the living analogues of which we have to look among the river shells of distant climates. On the contrary, there are diffused almost through every part of it innumerable specimens of genera which abound in the stagnant waters of England. To the details published by Mr. Webster (Geol. Trans. ii. 226, &c.), we have very little to add. In the higher part of the hill above Totland Bay, we found among the beds of compact limestone the casts of one or two species of land-shells, and of a large turbinated shell which we have not seen described. Immediately above the limestone, there is a thin bed of clay containing laminæ of coaly matter, and many shells, of the genus *cyclas*, in a state of perfect preservation, and with their valves united. As the same species is very abundant in some parts of the *upper marine formation*, we imagined during our first visit to the island, that we had discovered the trace of a *marine* deposit over the highest *freshwater* beds. The facts already stated prove that the supposition was devoid of foundation. Indeed we may hope that difficulties which at first appeared insurmountable in classing the tertiary rocks will gradually disappear as inquiries respecting the genera of freshwater shells are more widely extended. The *formation* may be traced on the north side of the road between the villages of Freshwater and Calbourne; but it does not extend very far to the north, nor has its eastern termination been well ascertained. It may be *expected* on the north side of the chalk range between Newport and Whitecliff Bay: its existence, however, in that region has not been yet ascertained. All the blocks we have seen near Bembridge and Whitecliff Bay are decidedly derived from the *lower freshwater formation*. Of the blocks which are scattered

about the surface in the northern parts of the island, near Cowes and Ride, we cannot speak with the same confidence, though they may, perhaps, generally be traced to the same *formation*. From all these facts we conclude, that the *upper* freshwater rock does not occupy so large a superficial extent as has been imagined.

On a review of all the phenomena presented by the tertiary deposits of the Isle of Wight, I see no good reason for altering their present classification—a conclusion which every one must arrive at with pleasure who properly estimates the scientific researches of Mr. Webster. At the time he commenced his examination of the Isle, little was known respecting our newer strata, nor had any one successfully attempted to identify them with the similar *formations* of the Continent. In proof of the truth of this assertion, we have only to recollect that there appeared in 1811, under the sanction of the Geological Society, a paper by Dr. Berger, in which the strata over the chalk were confounded with the strata under the chalk; and in which the Portland oolite was, by a magical power of misarrangement, made to represent the *calcaire grossier* of the Paris basin!

VIII. Diluvium.

This paper has already extended to so great a length that we shall omit some of the observations we intended to offer on the gravel beds of the district. It is true that in many parts of the Isle the external form of the country has little connexion with its physical structure. The accumulation of *diluvium* may, however, be sometimes traced to specific denudations. Thus we find that the masses of gravel which are so much accumulated to the north-west of Newport are connected with one of the greatest denudations exhibited by the whole chalk range. The denudation of Brading is, perhaps, still more remarkable. The small rivulets which spring on the south side of the central range do not descend into the sea by the way which nature seems to point out to them; but cut directly through the chalk downs, and find an outlet in Brading harbour. This fact proves that these rivulets have not excavated their present channels, and that the greater inequalities presented by the surface of the Isle have not arisen out of the long continued action of those destructive causes which are now in operation. Many other parts of our great chalk range present the same phenomenon.

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P. S. The fifty-ninth number of Sowerby's Mineral Conchology has appeared during the passage of the preceding paper through the press. It is now attempted to arrange under the genus *potamides* a great many shells which have been considered as *cerithia.* The shells which I have described by the generic term *potamides* belong exclusively to the species *ventricosus* and *acutus,* figured in plate 341. In their external characters, they are sufficiently distinguished from *cerithia,* and they are so intimately associated with freshwater shells that it is hardly possible for them to have been of marine origin. An enumeration of the fossils subsequently figured by Mr. Sowerby (plates 339, 340), was intentionally omitted, as they were considered of doubtful origin; and, therefore, of no assistance in separating the *formations.* It is very important that those who collect fossils on the Hampshire coast should describe the localities more carefully than they have been in the habit of doing. *P. rigidus,* pl. 338, is probably derived from some of the sandy beds which separate the *London clay* from the *lower freshwater formation.* A few freshwater shells may be expected in such a *marine* deposit. To avoid all ambiguity, would it not be better to expunge the genus *potamides,* and consider all the species as cerithia? Those which are inhabitants of freshwater might be distinguished from the others by some epithet, which would answer the purpose better than the artifice of making a new genus without any new generic characters. The fragments of the *bulimus ellipticus* (Min. Con. t. 337, f. 2), were found in the highest bed of the *upper freshwater rock* of Headen Hill.

Article IV.

A New Method of hanging Sluice-Doors and Flood-Gates.

By W. Marratt, AM.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Liverpool.*

The usual method of hanging doors which are intended to keep up or let out water from sluices, dams, &c. is either to place them so that they may be opened like common doors, or else to elevate and let them down by machinery; in both cases, the operation is often very tedious and troublesome. The following method, which, for any thing that I know, is new, and has not yet been put in practice, is certainly in many cases preferable to the methods just alluded to.

Let a rectangular door be fitted to the place for which it is designed, and let it be hung, by placing across it strong gudgeons, which must turn in holes made in the jambs, or in a wooden frame placed for the purpose; or they may play in circular holes made in the stone work. The proper situation for the gudgeons on the door may be thus determined: Draw two vertical lines, the whole length of the door, and find the *centre of pressure* of the door, corresponding to the situation the door is to have in the water; that is, according to the depth to which it is to be immersed; lay off on these vertical lines the distance of the centre of pressure from the upper end, and draw a line across the door through the centres of pressure; this line may be called the *line of pressure*. Place the axes, or centre lines, of the gudgeons so as to coincide with this line, and the gate will turn upon the gudgeons, and keep up or let out the water as occasion may require.

We may observe first, that such a door can be opened with the smallest force possible; that it will retain or keep any position in which it may be put; and, consequently, that any quantity of water may be evacuated at pleasure. It may also be closed again with the greatest possible ease; for the pressure of the water above and below the axis on which the door turns, being in all positions of the door equal, any effort which is sufficient to overcome the friction of the several parts will be all that is required either to open or shut the door.

This mode of hanging sluice doors will be extremely useful and convenient in situations where the sluice is liable to be choaked up with sand; in such situations much labour and expense are often required to open the doors when hung in the common way; but, according to this method, the door will be easily raised to a small height, which, being effected, the water will soon make its way, and carry away the sand along with it.

Gates hung in this manner would be the best of any for the head of a *mill-course*, and the practical engineer will easily determine other situations in which they may be used with advantage. As these doors or gates must always remain across the river, or sluice way, it is obvious that this mode cannot be applied where large vessels have to pass in and out; it may do, however, for small craft, and in every situation where navigation is not concerned, this mode of application is manifestly superior to the method now in use.

Where the upper end of the door is even with the surface of the water, the distance of the line of pressure from the top of the door is two-thirds of the length of the door; in every other situation, whether the top of the door be above or below the surface of the water, the centre of pressure coincides with the centre of oscillation, and is not difficult to determine. Tables for the use of mechanics might be easily constructed, if this method should be generally adopted in those situations for which it is best suited.

ARTICLE V.

Reply to C's Observations on Mr. Herapath's Theory.

(*Concluded from* p. 296.)

C. now sets himself about refuting Mr. H.'s theory of collision. A very few words will be sufficient to display on this point the "distinguished excellence of C.'s beautiful reasoning! conclusive arguments! invincible demonstrations! as self-evident as that two and two make five." C. admits that Mr. H. is correct in his Prop. 2, *Annals* for April, 1821. He allows that "bodies act with a force equal to their momentum;" and, therefore, as one consequence, that *the force with which a hard fixed plane and a hard ball moving perpendicularly upon it come in contact, is equal to the momentum of the ball.* Again, C. grants that "*the intensity of the force with which two hard balls moving in opposite directions come in contact is equal to the sum of their momenta.*" Admitting, therefore, that the three momenta in these two cases are respectively equal, it is evident, by what C. himself allows to be true, that the intensity of the collision in the latter case is double the former. Now whether the changes of motion be equal to the whole or only to half the intensities of collision, or even to a certain part of the intensities, it is on all hands allowed, I believe, in the case of perfectly hard bodies, that the changes of motion have at least the same ratio as these intensities. For instance, if a certain intensity of stroke produce a certain change of motion, double, treble, &c. that intensity will generate a double, treble, &c. change of motion. Therefore, in the case of the hard body and plane, the change of motion in the body is the half by what C. admits to that in either of the two movable bodies. Consequently if, as C. asserts, each of the two bodies just lose the whole of its motion by the stroke, the body striking on the plane will lose only half its motion; and, therefore, after the stroke, it will *proceed right through the fixed imperviable plane,* with the other half motion which remains to it! Such a consequence as this; such a refutation of Mr. H.'s theory, is well worthy the profundity of C.; and undoubtedly "makes it as self-evident as that two and two make five, that Mr. H. has in truth *quite* mistaken the road to philosophical science."

It is true C. does not say that the body will pass through the plane. He indeed tells us that Mr. H. is right in saying the body will remain at rest on it. The conclusion, however, which I have drawn is a legitimate consequence of what he grants and admits; and such I will venture to say that he will get no man of respectable scientific ability openly to contradict. Probably this reasoning may "not quadrate with" C.'s notion of induction. Should this be the case, it must be considered that C. does not work by the ordinary rules of philosophizing; and, therefore, unless he employs some preferable means to sanction his inductive asseverations, he must pardon common capacities for distrusting a system so very comprehensive as to prove truth error, wrong right, and, perhaps, even black white.

Besides what I have shown, we have other equally happy consequences flowing from C.'s physics that would be not a little amusing if we had time to pursue them. Of these, I shall merely select the following two or three, which will set C.'s depth and knowledge of the subject in question in the most advantageous point of view. C. says "that the intensity of the stroke between two bodies moving towards opposite parts is equal to the sum of their momenta;" and, therefore, when one of them is at rest before the stroke, the intensity must be equal to the momentum of the other. These propositions precisely coincide with Mr. Herapath's. Moreover, C. says that a hard body striking a hard fixed plane perpendicularly acts with a force equal to its momentum. This force is evidently the intensity of the stroke. Hence, therefore, the momenta in both cases being equal, the intensities of the strokes, and consequently the effects of these intensities on the motions of the bodies are equal. But C. tells us the one body after the stroke remains at rest on the plane; therefore, the other body striking the quiescent one likewise remains at rest after the stroke. Now, though this agrees with Mr. H.'s theory, it is decidedly at variance with the old. The old theory makes the two bodies after the stroke to go on together; and hence the collision deprives the striking body of only a *part* not of the *whole* of its motion. C. has consequently embraced views in direct opposition to the theory he means to advocate; and that too in the very elementary parts of it; and what makes it better than all in the elementary part of a subject, "whose principles," he tells us, "are as nearly as possible self-evident." It is not, I think, in the power of C. or any person whatever, to refute Mr. Herapath, or overturn the theory of heat of our illustrious Newton. Let me remind C. that it is of no avail to attempt to annihilate theories which have been fairly deduced from facts, by mere assertions. Indeed I entertain some doubt whether C. clearly understands the theory which he has undertaken to advocate.

From the examples I have given, an estimate might easily be made of the value of the rest of C.'s observations. I might hence be very well excused from attending to his other remarks; but lest he should conceive I dismiss them too hastily, I will accompany him a step or two further.

Mr. Herapath, in his theory of collision, says, "if a hard ball or other hard body be held against a fixed hard body or plane, and in this way receive the impulse of another body," the force with which the one side of the intermediate body is driven towards the other is equal to the momentum of the moving body. In proof of this, Mr. H. argues that "the fixture being at rest, the part of the intermediate body which is against it cannot be urged any way by the fixture; and, therefore, the force with which the moving body comes in contact with the other side; that is, the momentum of the body, is the force of constipation." But C. says, "the *two* surfaces of the intermediate body will be urged *towards* its centre," in consequence of the reaction of the fixture, "with a force exactly as great as if each side had been struck with a momentum equal to that of the moving body." Thus instead of the centre being urged towards the fixed plane, which merely opposes a passive resistance, this quiescent plane, according to C. *drives* the side it is in contact with *towards* the centre. Will C. have the goodness to tell us how this takes place? Will he be kind enough to explain to us how and in what direction a quiescent and a fixed body can actively urge another without elasticity? But he informs us the thing can be proved by experiment. No doubt C. has made this experiment, and will immediately favour the world with it. A great treat I am persuaded it will be to our men of science. As an humble admirer of scientific truth, I shall myself feel highly gratified and obliged. In the interim, however, I cannot help saying, that had I seen an experiment producing such a result, I should have much questioned the fidelity of my senses.

"If," says Mr. Herapath, "two perfectly hard bodies, moving in the same right line, but towards opposite parts, come in contact, the sum of their momenta being the motion with which the two bodies approach, is, therefore, the motion or force with which their surfaces come in contact." This, C. has "the dignified condescension" to admit. "But" continues Mr. H. "the force with which the surfaces come in contact is the force with which each surface, or body, is acted on at the time of the contact in a direction opposite to that in which the body was moving." Nothing surely can be more evident than this; and, therefore, to have attempted to explain or illustrate so obvious a thing would have been to offer an insult to the understanding of his readers; particularly when we consider that these readers were to be the members of the Royal Society, who are reputed to be men of talent and ability. Nevertheless, C. says "he is at a loss to discover how it can be proved," notwithstanding he allows that the intensity of the stroke is equal to the sum of the momenta. What, I would ask C. is meant by the intensity of the stroke but the violence of the contact? And what is this violence of contact but the force with which each surface is struck? For example, if I strike a nail with a hammer, the momentum with which the hammer comes in contact with the nail measures, and is just equivalent to the violence or intensity of the blow on the nail; supposing both bodies absolutely hard.

This intensity of impulse is equally felt by the hammer, not from any *vis viva* of reaction in the nail, but from what may be termed a passive opposition to its motion. The same also must evidently hold good in the intensity of the stroke between two bodies moving towards opposite parts; each of the bodies like the hammer and nail receives an impulse equal to the whole intensity of the contact. However, since C. finds a difficulty in this case, to oblige him I will try if I can put it more simply than Mr. Herapath could well be expected to do, when writing to the Royal Society. And to prevent C. from confusing himself by attending to more than one idea at a time, I will endeavour to demonstrate the separate steps in separate propositions; taking care, for like reasons, to make the proofs, as far as I can, analogous to those notions in the old theory to which I dare believe him he has paid much attention, though unfortunately with, as I have shown, but little advantage.

Prop. A.

If two perfectly hard and equal balls at rest be similarly struck by two other perfectly hard balls moving with equal momenta, the intensities of the strokes are equal.

For because the two bodies struck are perfectly hard, equal, similar, and quiescent, and the strokes similarly given, no difference on either of these accounts can be made in the intensities of these strokes. Whatever difference exists must, therefore, depend on the momenta and the manner in which the impulses are communicated. But all the bodies being absolutely hard, the strokes are mere impulses, which are begun and finished with the very commencement of the contact; and are, therefore, equally smart with respect to duration under every velocity. Hence the velocities of the moving bodies have no effect on the intensities of the strokes, all other things being alike. The momenta, therefore, alone influence the intensities of the strokes; but in the present case the momenta are equal; the intensities consequently are equal.

The substance of this theorem appears in Mr. H.'s Cor. to Prop. 1, *Annals* for April, and is distinctly mentioned and made the foundation of Mr. Herapath's demonstration of his Prop. 3, though C. in his parody of this demonstration has, notwithstanding its evidently indispensable importance, descended for the purpose of suiting his own views, to an artful omission of it.

Prop. B.

If two perfectly hard, equal, and quiescent balls be similarly struck by any two other perfectly hard balls, the intensities of the impulses will have a ratio equal to that of the generating momenta.

By the preceding Prop. if the momenta were equal, the intensities would be equal. And by the demonstration of this same proposition, it appears, that because all the bodies are perfectly hard, and the two quiescent bodies perfectly equal, and the strokes similarly given, the intensities of the impulses are due to the momenta alone. But other things being alike, causes are proportional to their effects; and the effects of the momenta in this case are the intensities of the impulses. The intensities, therefore, are proportional to the momenta.

Cor.—Because the unconstrained changes of motion are proportional to the intensities of the impulses, the changes of motion both in the bodies striking and bodies struck, are proportional to the momenta.

Before I proceed further, it will be needful, in order to ease C.'s mind of apprehension, to show him that the preceding Props. are perfectly compatible with the notion introduced into his favourite, but little understood, theory of collision. This I shall do by a few quotations. "The force of percussion is the *same as the momentum or quantity of motion*, and is represented by the product arising from the mass or quantity of matter moved, multiplied by the velocity of its motion; and that without any regard to the time or duration of action; for its action is considered *totally independent of time*, or but as for an instant, or an infinitely small time."—(Hutton's Mathem. Dictionary, vol. ii. p. 169.)

"Bodies that have equal quantities of motion have *equal forces* or *equal powers, to produce* motion."—(Playfair's Outlines of Nat. Philos. vol. i. p. 32.)

"The momentum, or quantity of motion, generated by a single impulse, or any momentary force, is as the generating force."—(Hutton's Courses, vol. ii. p. 132.) In the same page the same writer says: "The velocities being equal, a double mass will strike with a double force; a triple mass with a triple force; and so on."

These quotations not only confirm the two preceding Props. but the first confirms the principles on which they are founded; namely, the evanescent continuance of the strokes. What makes it the more extraordinary is, that these are two of the principal authors whose works C. wants to oppose to Mr. Herapath.

M'Laurin's Fluxions, in which I believe his views of collision are expounded, I have not by me. If I had I should probably be able to give another amusing specimen of C.'s knowledge of names instead of things; but I will now beg leave to make one more quotation from another of our mathematicians, whose honest opinion in this matter may be entitled to some attention, even if it be not supported by the discovery that Newton in his theory of heat "has quite mistaken the road to philosophical science."

"If a body striking another gives it any motion, *twice* that body striking the *same* with the *same* velocity will give it *twice* the motion, and so the *motion* generated in the other will be as the *force* of percussion."—(Emerson's Tracts, p. 13.)

I shall make no further observations on the coincidence of the preceding Props. and the quotations; let C., if he can, show the difference. Let him also tell the world what he himself means by the following passage in his paper, *Annals* for Dec. p. 421; and let him point out which or what part of the preceding Props. it refutes: "Now bodies act with a force *equal* to their momentum." If C. cannot do either of these things, perhaps he will have the goodness to clear up the following difficulty, or paradox, which has perplexed me a little in his favourite doctrine. Let a perfectly hard ball, A, moving with any velocity, *a*, strike in the line of its motion another perfectly hard ball, B, at rest; then by the old theory the motion of B after the impulse, or the motion it acquires by the stroke, $= A\,a - \frac{A\,a}{A+B}\,A = \frac{A\,a\,B}{A+B}$; and in any other parallel case the motion acquired by the same B at rest $= \frac{A'\,a'\,B}{A'+B}$. Now by the views in the quotations I have made from Hutton, Playfair, Emerson, and C. himself, it is evident that if the momenta $A\,a$ and $A'\,a'$ were equal, the intensities of the strokes and momenta due to the body B after the strokes would be equal. That is, $\frac{A\,a\,B}{A+B} = \frac{A'\,a'\,B}{A'+B}$, or $A = A'$, however unequal the values of A and A′ may be. In other words, if the theory and quotations be both correct, there *cannot be* a number *greater* or *less* than unity. Would C., the unsolicited friend and voluntary champion of the old theory, be kind enough to unravel this scientific enigma? I need not exhort him to embrace so excellent an opportunity of displaying, without equivocation and subterfuge, and without any paltry attempt to evade, the true merits of the theory he professes so well to understand. As the principles of the theory for which he voluntarily, I will not say unnecessarily or officiously, throws down the gauntlet "are as nearly as possible self-evident," it will not, I presume, require any time or reflection in him to explain this matter. In next month's *Annals*, therefore, I hope he will, for the credit of himself and theory, mathematically clear it up; and thus expose the fallacy of what, perhaps, he will readily demonstrate to be "mere figments of the imagination." Should, however, a want of leisure prevent his complying with my request at so early a period as I have named, let him only say in the next number that he will do what I require, and I will patiently wait any time that he pleases.

Prop. C.

If a perfectly hard ball strike another perfectly hard ball at rest in the line described by the centre of gravity of the former, the striking body will remain at rest after the impulse, and the other will proceed in the same right line in which the former was moving, and with the same momentum.

All that I require for demonstrating this Proposition is, *that the intensity or force of percussion be the same as, or equal to, the motion generated; and that the force of percussion be proportional to the generating momentum.* Without adverting to the preceding propositions, each of these postulates is admitted in the quotations I have made from the authors C. has quoted against Mr. Herapath. I shall, therefore, not trouble myself about their accuracy, which is indeed "as nearly as possible self-evident," but shall proceed with the rest of the proof. Let B, B′, be two perfectly hard and equal balls at rest, and let A, A′, be any two other perfectly hard balls striking respectively B, B′, according to the conditions of the proposition. Let also a, a', be the velocities of A, A′, before the strokes, so that $A\,a = A'\,a'$. Then if b be the velocity of B after the stroke, and b' that of B′, we have $B\,b = B'\,b'$, and $b = b'$. Now if A move at all after the stroke, it must follow the body B with an equal or less velocity than b; because it could not move the contrary way unless the force of percussion was greater than the generating momentum, which is impossible. The same is likewise true of the body A′. Therefore, if they do not remain at rest, let them follow B and B′ with the velocities p, p', respectively. Then because the sum of the momenta in each case before and after the stroke is the same $A\,a = A\,p + B\,b$, and $A'\,a' = A'\,p' + B'\,b'$, and consequently $A\,p = A'\,p'$; that is, the velocities p, p', remaining to A, A′, after the strokes are reciprocally proportional to the bodies A, A′. Suppose $A = B$, then p will be a certain part, for instance, the nth part of b, so that $n\,p = b$. Therefore $b = n\,p = n\,p'\,\frac{A'}{A}$ and $p' = b'\,\frac{A}{n\,A'}$. Now the value of $\frac{A}{A'}$ may be any thing we please, and, therefore, much greater than n; in which case p' must be greater than b'; that is, if p has any magnitude, the body A′ which cannot move faster than B′, because it comes behind it, might nevertheless have a greater velocity in the same direction, which is absurd. Therefore, p and p' must each be equal to o, the only case in which the equation $A\,p = A'\,p'$ can be universally true; or both the bodies A, A′, must remain at rest after the impulses, and, consequently, the bodies B, B′, proceed with the momenta $A\,a$, $A'\,a'$, respectively.

Cor. 1.—Because the intensity of the stroke is equal to the momentum communicated, and this momentum is equal to the momentum of the moving body before the stroke: this momentum of the body before the stroke is equal to the intensity of percussion; and the whole of this intensity must be equally felt by each of the bodies without any regard to their relative size.

Cor. 2.—Hence also the velocities of the bodies in motion before and after the stroke are reciprocally proportional to the bodies.

Cor. 3.—And because the momentum communicated is equal to the momentum of the moving body before the stroke without respect to the relative magnitudes of the bodies, it follows that a double, treble, &c. quantity of motion will generate a double, treble, &c. quantity of motion, not in the same quantity of matter only, but in any quantity.

This, though at variance with the results of the old theory, as I have shown above, precisely coincides, in the case of the same quantity of matter struck, with the views in some of the above quotations from Dr. Hutton and Emerson. But the following declaration in Hutton's Dictionary, vol. ii. p. 170, puts it beyond a doubt that results from Mr. H.'s theory accord with the usual opinion of mathematicians on this subject. " Now it is a law," says Dr. Hutton, " *universally allowed* in the communication of motion, that when different bodies are struck with equal forces, the velocities communicated are reciprocally as the weights of the bodies that are struck." Therefore, if it be true, as the same writer says in one of the preceding quotations, " that the velocities being equal, a double mass will strike with a double force, &c." we want no further evidence that Mr. Herapath's theory furnishes consequences " which have been admitted as incontrovertible by the ablest mathematicians in all ages."

The demonstration of this Prop. and its Corollaries, it will be seen, is rigorously mathematical from data which have been admitted by decided advocates for the old theory,—the very authors C. has opposed to Mr. Herapath. Nothing more, therefore, need be advanced in support of the proposition; but I might observe that I could here subjoin a rigorous proof similar to the one C. has, by leaving out certain principal parts, parodying others, and unhandsomely offering them to the world, as C. has done with Mr. Herapath. Taking, however, no further notice of this part of the affair, I cannot but compare the reasoning C. tells the world is Mr. Herapath's to a picture whose intention the artist thought it needful to explain by writing under it, " *This is a Cow;* " lest it might be mistaken for any thing else.

From the views here developed, and the constitution of aeriform bodies, as laid down by Mr. Herapath, if a body composed of absolutely hard particles mutually impinging on one another in the way Mr. H. has assumed in his theory of heat, be projected in such a body as our air, it will proceed with a velocity gradually diminishing on account of the resistance it continually experiences from the opposition of the air. For a single particle may, from Mr. H.'s principles of collision, be stopped, or even driven backwards, by the first particle it met with at rest, or moving in the opposite direction. In any compound body, however, it is only the superficial particles of the body the particles of the air strike against; and since the particles of the body are not fixed to, but freely moving among, one another, except inasmuch as they are prevented from flying off indefinitely by their mutual attraction, the intensities of the collision between the particles of the body and particles of the air are merely equal to what would arise if the former particles were free and disengaged, and moving with the same velocity as the body of which they form a part. These intensities, therefore, and the effect which they have on the progress of the body, are by no means the same as they would be if the particles of the body were firmly and inflexibly united, or the body itself one perfect solid. Though this consequence is one of the most obvious that can be, C. has raised a "wonder how the cannon balls with their hard particles can get on, when they strike the hard particles of the atmosphere in the lines of their centres of gravity." Perhaps the greatest wonder is, how so acute a reasoner as C. should have published an objection which evidently has no foundation to rest upon.

After what I have shown of the merit and weight of C.'s observations, and Mr. H.'s principles of collision, he will, perhaps, take it kindly of me if I let alone his "pin's head" difficulty. I must confess I am very much disposed to oblige him; and, therefore, will leave the wisdom of one head to solve the phænomena of the other. But I must beg leave to tell him that Mr. Herapath had minutely considered this objection, and clearly answered it in the very number of the *Annals*, and only five pages after the Proposition from which C. would make us believe he had the sagacity to draw it. Would C. have the goodness to tell us whether the discovery of this consequence is due to his own penetration; or whether he has brought forward the objection Mr. Herapath had himself raised, and artfully omitted to notice Mr. H.'s explanation, for the purpose of undermining a theory which prejudice would not allow him to admit? Besides, what I have mathematically deduced in Prop. C. Cor. 3, from the principles "which," C. tells us, "are as nearly as possible self-evident," Mr. Herapath has distinctly shown, p. 292 and 293, *Annals* for April, 1821, "that the whole difficulty of the case turns on the abstraction of the ideas of magnitude and momentum." But I believe I have promised C. not to pursue this part of his objections. I will, therefore, desist.

Prop. D.

If two perfectly hard and equal balls come in contact, when moving with equal momenta in the same right line towards opposite parts, the intensity of the stroke as felt by each body in a direction opposite to that in which it was moving, is equal to the sum of the momenta of the two, or twice the momentum of either one before the stroke.

Mr. Herapath has demonstrated this theorem generally in his Prop. 5, *Annals* for April, 1821. I have chosen this particular case of it, because against this, C. has levelled his objections; and in the proof I intend to have recourse merely to what I have already demonstrated from the principles admitted in the old theory, and to a result which agrees equally well with both theories.

By the old theory, if a hard body A, having the velocity a, strike directly another hard equal body A′ at rest, the motion communicated to A′ by the impulse is $\frac{\mathrm{A}\,a}{2\,\mathrm{A}}\ \mathrm{A} = \frac{\mathrm{A}\,a}{2}$. And by the same theory, if the two same balls meet each other, instead of one of them being at rest, with equal opposite momenta A a, A′ a', the motion destroyed in either, or, which is the same, the motion communicated to either is A a. But by the quotations I have made from C.'s quoted authors, these communicated motions are equal to the intensities of their respective strokes felt by each body in the direction in which the motion is communicated. Therefore, the intensity of the stroke on either body when one is at rest, is half as great as when both meet with equal opposite momenta. Now when one of the bodies is at rest, I have shown, Cor. 1, Prop. 6, by strict mathematical reasoning from the principles admitted in the old theory, that the intensity of the stroke on each is equal to the momentum of the moving body; when, therefore, they are both moving with equal momenta towards opposite parts, the intensity of the stroke on each is equal to twice the momentum of one, or the sum of the momenta of the two.

Cor.—Hence the two equal bodies after the impulse recede towards the parts whence they came with the same momenta they had before they met. For the motion communicated by the impulse is equal to the intensity of the stroke on the body, and this intensity is equal to 2 A a; but at the time of the stroke, the body had a momentum in an opposite direction equal to A a. Therefore at the time of the contact, the body is the same as if it was urged in two opposite directions by the forces A a and 2 A a, the former impelling it in the direction in which it was moving, and the latter on the contrary; consequently it retraces its path with the momentum A a.

This conclusion, brought out by strict mathematical induction, from the principles of the old theory, coincides with Mr. Herapath's, and also with the theories of Wren, Huygens, and Wallis.

It is worthy of remark, that C. by way of *mathematically* refuting this conclusion, admits the principle of Mr. H.'s proof, that the intensity of the stroke is equal to the sum of the momenta, but denies the consequence, without showing why, or assigning any other reason, than that he is "at a loss to discover" how it follows. We shall, perhaps, not lose our pains in transcribing what C. says on this subject; for if we can derive no information, the consideration of it will afford us amusement. "How," says C, "Mr. H. proves that the intensity of the stroke is the force with which each of the balls is acted on in a direction opposite to that in which it came at the time of the contact, *I am at a loss to discover.*" "The *intensity* of the force," observes C. "is equal to the *sum* of the momenta with which both balls *come* in contact, *half* of which is *one* direction and *half* in the *opposite.*" Here C.'s *mathematical* refutation of Mr. H.'s theory amounts to this—he cannot see how it is, nor how it is not. But the beauty of all lies in the elegant, the decisive, the irrefragable mathematical demonstration with which he establishes his counter proposition. He tells us "half the intensity is one direction, half in the opposite;" and he proves it—how? —not mathematically, not by common legitimate induction; but without a fact—without a circumstance, nay even without a word either for it or against it. This mode of procedure is perfectly consistent with C.'s general method, but let me again remind him that mere assertions are totally insufficient to overturn well-founded reasoning.

"If," says C, "a man *push* with all his strength against a wall, say with a force of 10, action and reaction being equal, the wall resists with a force as 10. If, instead of the wall there be an opposing active force, another person, for instance, *pushing* against the first with an exactly equal force," "*by Mr. Herapath's reasoning, each person would be acted on in a direction opposite to that towards which he pushed, by a force equal to twice the force of either one;* that is, with a force of 20, and consequently both must be pushed backwards." These sentences, as far as I understand them, distinctly charge Mr. H. with confounding pressure with impulse, and with applying the laws of a single impulse between perfectly hard bodies to a pressive force. Flatly to contradict this, and to challenge C. to produce only one expression of Mr. H. corroborative of such assertions, would be to raise this attempt to depreciate and misrepresent Mr. H.'s labours to an importance to which not even the best of C.'s objections seems entitled. I shall, therefore, merely quote a passage or two from Mr. Herapath's papers declarative of his opinion on pressure and impulse; and then leave C. to compare them with his own assertions.

"It is manifest from the drift of it," (a passage in Mr. Tredgold's attack) "Mr. T. can compare pressure with impulse. Of course, he can also compare a mathematical line with an area; and thence tell us how many lines there are in a superficies, how many superficies in a solid; and, as a finale, I expect how many inches in an hour."—(Mr. Herapath's Reply to Mr. Tredgold, *Annals* for Dec. 1821, p. 464.)

"Had I *** it would have been like endeavouring to equate a single impulse with an unceasing force," (pressure he means) "for an indefinite time—a manifest impossibility."—(Mr. H.'s Reply to X. *Annals* for January, 1822, p. 30.)

C. tells us that the "*pushing*" case I have just quoted, which (with how much truth the reader may judge from the counter quotations), he informs the world, is Mr. Herapath's, is that by which it is intended by Mr. H. that "the doctrines of Newton, Maclaurin, Hutton, Playfair, and innumerable other mathematicians, are to be overturned in relation to the collision of hard bodies." We have already seen that C. has not been over fortunate in quoting, nor very happy in understanding, the writings of Dr. Hutton and Prof. Playfair, though it be true they deliver "principles as nearly as possible self-evident." In the present instance, I think, we shall find he has not been more successful in his acquaintance with Sir I. Newton. However, it is necessary for me to premise, that I am not aware Newton has said any thing of collision, except in the first part of his Principia. If he has in any other places, I shall be happy to be corrected; for I do not recollect to have seen it. Even here he has given nothing in the shape of regular argument: a few loose ideas only, thrown in apparently more by accident than design, are all I can perceive. They happen, however, to be of that peculiar cast as to satisfy us that C. has either not seen them, or not understood them; or what, perhaps, is more probable, that his zeal to oppose Mr. Herapath has outstripped his discretion and his knowledge; and hence occasioned him to quote authors without knowing what they have written.

In the Schol. Cor. 6, Principia, Third Law of Motion, Newton says: "By the theory of Wren and Huygens, bodies absolutely hard *return* from one another with the same velocity with which they meet." This was the case with the theories of Wren and Huygens when the bodies were supposed to be perfectly equal; and, therefore, instead of coinciding with the doctrine of collision C. advocates, it coincides with Mr. Herapath's. That Newton was not here confounding hard with elastic bodies appears from the sentence immediately following the above, and implying that though he did not question the truth of this case of Wren and Huygens's theories, yet the evidence of it was greater in elastic bodies. "But this may be affirmed," says Newton, "with more certainty in perfectly elastic bodies."

To satisfy us what his opinion was, he says in the same Schol. "By the same" (first and second Laws of Motion), "together with the third Law, Sir Christopher Wren, Dr. Wallis, and Mr. Huygens, the greatest geometers of our times, did severally determine the rules of the congress and *reflection* of hard bodies and much about the same time communicated their discoveries to the Royal Society, exactly agreeing among themselves as to those rules. Dr. Wallis indeed was something earlier in the publication; then followed Sir Christopher Wren; and, lastly, Mr. Huygens. But Sir Christopher Wren *confirmed* the truth of the thing before the Royal Society, by the experiment of pendulums, which Mr. Mariotta soon after thought fit to explain in a treatise entirely upon that subject." Nothing can be plainer than this; and, therefore, nothing more evident than that the theories of Wren and Huygens were those which the best philosophers of Newton's time embraced, and which Newton himself looked on as established by the experiments of Wren. Now it unfortunately happens for C.'s assertions and objections, that the two principal cases of Wren's theory for hard bodies; namely, that wherein the equal bodies meet with equal opposite momenta, and that wherein * one of them is at rest before collision, exactly coincide with Mr. Herapath's. Mr. H., therefore, instead of standing opposed to Newton and "the ablest mathematicians in all ages," has in the two leading cases of his theory the expressed testimony of no less than Wallis, Wren, Huygens, and Newton; besides Mariotte, and probably a number of other respectable mathematicians. But the most extraordinary circumstance is, that the present doctrine of collision, which has evidently crept into existence since the days of Huygens, Newton, &c., C. unequivocally gives us to understand is that which has been embraced by "the ablest mathematicians in *all ages*," notwithstanding here is indisputable evidence that a different theory was maintained not 100 years since by the first mathematicians the world has yet produced. How would C. wish us to dispose of this new article? Shall we debit his accuracy or his knowledge with it? Shall we lay it to his insinuation that Mr. H.'s theory cannot account for the phænomena of latent heat; to his assertion that Mr. H. makes hardness and elasticity the same; to his parodying of Mr. H.'s theorems for the purpose of misrepresenting them; to his charge that Mr. H. confounds pressure with impulse? Or shall we add it to his knowledge of Newton's theory of heat; of Hutton, Playfair, and Emerson's principles, and of Wallis, Wren, Huygens, and Newton's theories of collision? I will not, however, "*push* C. to the wall" on this subject.

I have now replied in detail to C.'s objections, &c. to Mr. Herapath's theories of heat and collision; and I have shown that he has not advanced a single circumstance tending to weaken, much less to invalidate, any one of Mr. H.'s views. Indeed in the whole that C. has said, there is not, one would imagine, even an attempt mathematically to refute Mr. H.'s propositions, or to show that his reasoning is erroneous or illogical; for though Mr. Herapath has dealt so largely in numerical facts, has developed so many laws, has compared his theory with so many experiments, and has predicted the phænomena of so many new and untried cases, yet C. has not ventured to question a single fact, to refute a single law, to invalidate a single experiment, or to disprove one solitary phænomenon either advanced or predicted. We might in truth say, the whole of C.'s attack exhibits a man struggling with a subject to which he is unequal, or with which he is unacquainted; yet who would like to say something if he could; who is clearly in possession of the Will to refute, and as clearly in want of the Power. Hence we see misrepresentations for arguments, unsupported assertions for proofs, errors for facts, and ingenious quibbling for sound reasoning. To form an opinion, however of Mr. H.'s labours, and of the probability of his having succeeded in the great objects of his inquiry, let any one who is capable of judging examine the coincidences of his investigations with facts, collected in the *Annals* for Jan. 1822; let him look at the simplicity of the principles, attributing to matter only two properties, hardness and inertia; let him afterwards consider the number, extent, variety, and apparent incongruity, of the experimental testimonies adduced; and then let him say, not whether it is probable Mr. H. has succeeded, but whether it is possible he can, with such corroborations, not have succeeded. On any opinion thus formed, and given by minds competent to judge, and liberal enough to acknowledge conviction, Mr. Herapath may with safety rest his credit and his fame.

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* I cannot speak positively as to this case; but unless the impression of it in my mind has changed since I read it in the Phil. Trans. it is precisely as I have stated.

On the subject of Mr. H.'s connexion with the Royal Society, into which, I think, C. has imprudently entered, I shall at this time say nothing. If Mr. Herapath's labours stand the test, the Royal Society will find, even among those who now support them, if they have acted improperly or illiberally, enough to blame and to censure them; and if their conduct has been correct, or marked with liberality and encouragement, Mr. Herapath himself, I presume, will not be among the last to acknowledge it.

I have now only to request that if C. answer this reply, he will do it candidly, and not evade or avoid the absurdities I have pointed out, both in his own arguments, and in the theory for which he commenced the attack. An open and honourable opponent, however sharp or severe, will always insure the respect, and generally the approbation, of D.

ARTICLE VI.

Meteorological Results of the Atmospherical Pressure and Temperature, Rain, Wind, &c. deduced from Diurnal Observations. Made at Manchester in the Year 1821. By Mr. Thomas Hanson, Surgeon.

Latitude 53° 25′ North. Longitude 2° 10′ West of London.

1821.	BAROMETRICAL PRESSURE.							TEMPERATURE.					RAIN.					WIND.										
													Manchest.		Ardk.	Lymm.	Cpsall.											
Months.	Mean.	Highest.	Lowest.	Range.	Greatest variation in 24 hours.	Spaces in inches.	No. of changes.	Mean.	Highest.	Lowest.	Range.	Greatest variation in 24 hours.	Inches.	Wet days.	Inches.	Inches.	Inches.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Variable.	Brisk.	Boisterous.
January	29·75	30·64	28·96	1·69	·66	3·10	4	40·3	55	23	32	15	1·095	6	1·324	1·045	1·703	0	7	0	5	1	8	3	2	5	1	0
February	30·11	30·52	29·20	1·32	·42	2·15	7	37·9	55	25	30	20	0·535	4	0·864	0·260	0·528	0	9	2	0	0	4	0	11	2	1	0
March	29·42	30·15	28·92	1·23	·67	5·20	11	44·7	61	29	32	36	2·625	18	3·145	2·947	3·876	0	3	0	0	0	17	6	3	2	1	3
April	29·51	30·00	28·96	1·04	·50	4·10	8	52·2	74	33	41	25	3·820	19	3·984	2·739	3·523	0	1	0	0	1	23	0	4	1	2	1
May	29·75	30·10	29·16	0·94	·40	4·10	6	52·6	73	34	39	30	2·520	15	3·194	2·252	2·884	0	1	0	2	3	13	1	9	2	0	0
June	29·97	30·28	29·55	0·73	·34	1·80	7	57·4	74	40	34	27	1·060	8	1·458	1·390	1·203	0	8	3	1	3	7	5	2	1	0	0
July	29·79	30·18	29·32	0·86	·40	3·20	7	61·9	81	44	37	23	1·905	9	2·496	1·143	1·872	0	1	5	4	0	0	16	5	0	0	0
August	29·80	30·08	29·25	0·75	·50	2·80	8	63·1	79	48	31	23	3·135	17	3·519	2·537	3·281	0	0	0	3	2	12	10	3	1	0	0
Sept.	29·69	30·10	29·16	0·94	·56	3·20	13	60·6	76	46	30	24	4·595	23	5·466	3·845	4·279	0	0	0	2	1	25	2	0	0	2	1
October	29·71	30·16	28·75	1·41	·63	5·20	10	52·4	66	39	27	26	2·900	20	3·287	2·270	4·323	0	0	0	5	1	20	5	0	0	0	1
Nov.	29·59	30·16	28·92	1·24	·67	5·00	9	47·8	62	35	27	17	4·390	22	5·400	3·771	3·442	0	1	0	5	0	16	5	2	1	1	2
Dec.	29·24	30·10	28·16	1·94	·56	7·80	15	43·8	62	31	31	23	3·800	19	4·971	3·880	3·864	0	7	0	6	0	14	0	1	3	1	2
	29·69	30·20	28·94	1·17	·53	47·65	105	51·2	68	35	33	24	31·880	180	39·108	28·087	34·778	0	38	10	33	12	159	53	42	18	9	10

The annual mean temperature of the past year is 51°; being about 2° above the average; the mean of the first three months, 40·9°; second, 54·1°; third, 61·9°; fourth, 48°; of the six winter months, 44·4°; six summer months, 57·9°. The maximum, or hottest state of the year, was 81°, which occurred on the memorable 19th of July, the Coronation of King George IV.; the minimum or coldest state was 23°, which is only 9° below freezing; this happened on the 4th January, making an annual variation of 58°. From the above, the reporter is enabled to draw the following comparison between the past and preceding year, viz. the average heat of the six summer months of 1821 was nearly one degree more than that of 1820, and the heat of the six winter months, 3° above the corresponding ones of the preceding year, so that the temperature of 1821 has been more mild than usual, and not marked by any very great extremes.

The annual mean elevation of the barometer is nearly 29 inches and 7-10ths; highest, 30·65, which was on January 23; lowest, 28·16, which happened on December 28; the difference of these extremes makes 2·49 inches: mean of the six summer months, 29·75; of the six winter months, 29·63. The mean daily movements of the barometrical surface measure near 48 inches: total number of changes, 105. The barometer throughout the month of February was remarkably high and desultory in its movements: on the contrary, in the month of December, it oscillated most extraordinarily; and towards the close of the year very low; the utmost depression was the minimum of the year.

Much has been said about the wetness of the past year. My annual account scarcely amounts to 32 inches in depth, which is certainly under the average for Manchester. Mr. John Blackwall, of Crumpsall, makes his annual fall three inches more, and Mr. John Dalton, for Ardwick, nearly eight inches more than mine. On the contrary, Mr. Edward Stelfox, of Lymm, near Warrington, has only registered a fall of 28 inches. The differences in our annual statements of rain, from places so near together, are singular, and certainly require an attentive inquiry. The only difference in our apparatus is, that Mr. Dalton's rain funnel is larger: mine, Mr. Blackwall's, and Mr. Stelfox's, are made alike, the same size, and of one material, which is that of copper. Provided our calculations of the method of measuring the rain collected in these funnel-areas be correct, and which I have every reason to conclude is the case; and provided their surfaces are parallel with the horizon, and at sufficient distances from trees, buildings, or any object that might obstruct a free access, it must follow that there can be no error in our results. I have noted down 180 days on which rain fell more or less, which number is one less than last year. In the last five months of 1820, there were 85 wet days; the number in the corresponding ones of 1821 is 101. February was the driest, and September and November the wettest.

The south, south-west, and west winds, have been the most prevalent: those winds were noticed to blow on 224 days. On the 18th, 19th, and 20th of March (about the vernal equinox), the wind blew hurricanes from the north-west, attended with rain, snow, and sleet. On the night of the 30th of November, and following morning, the wind blew a most violent gale from the south-west, accompanied with hail and rain; the damage done in consequence, by the falling of chimneys, unroofing of houses, &c. was great; several lives were lost in Liverpool and other places, and a large number of vessels suffered in the harbours and on the neighbouring coasts.

Bridge-street, Jan. 28, 1822.

Article VII.

On Blocks of Granite, Syenite, &c. imbedded on Diluvium.

By N. J. Winch, Esq. FLS. and MGS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Newcastle-upon-Tyne, March* 10, 1822.

Among the interesting phænomena serving at every step to arrest the attention of the geologist, there is one of ordinary occurrence in the north-east of England, which, I believe, is not as yet satisfactorily accounted for; and in hopes that some of your correspondents may be able to explain the true cause of a circumstance appearing to me to be enigmatick, I take the liberty of addressing you on the subject. Embedded in the diluvium of Northumberland, Durham, and Yorkshire, large blocks of granite, syenite, porphyry, greywacke, as well as of encrinal limestone, and basalt, are every where to be met with. That the granite, &c. &c. should have been transported by the agency of a powerful current of water from the cheviots or even from the mountains in the vicinity of the Cumberland Lakes, previous to the formation of the vale in which the river Eden flows, can readily be imagined; but the puzzling part of the fact is, that the loose earth in which these large and heavy masses are deposited appears to owe its origin to the strata immediately below it. For instance, on the red sandstone of the vale of Tees there is a red soil; and our porphyritic, basaltic, and encrinal limestone hills, are well known to afford a rich and fine pasturage, owing to the nature of the earths, resulting from the disintegration of these rocks. On the other hand, the coal field is generally covered by a strong clay mixed with a portion of sand evidently derived from the shale and sandstone on which it rests. The question to be resolved is, "Why the current of diluvian waters, possessing sufficient impetus to bring enormous fragments of rocks from great distances, did not denudate the strata of the light soil in which these masses are now embedded not only in the *lower* but in the *upper part* of the earth resting upon the more solid substrata." I remain, Sir,

Your most obedient servant,
N. J. Winch.

Article VIII.

On the Geology of the Eastern Part of Yorkshire.
By N. J. Winch, Esq. FLS. and MGS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Newcastle-upon-Tyne, March* 20, 1822.

The Rev. G. Young, and Mr. Bird, of Whitby, have just published a quarto volume on the Geology of the Eastern Part of Yorkshire, which, through the favour of a subscriber to the book, I have had an opportunity of perusing. Of the merit or demerit of the work, I do not feel myself inclined to speak, but as the authors have travelled out of their road for the purpose of writing strictures on two essays of mine printed in the Transactions of the Geological Society of London, I shall take leave to rebut the charges of inaccuracy and presumption brought against me by these gentlemen.

The first is comprised in the following note, p. 170: "Mr. W. is mistaken in asserting (vol. iv. p. 7, Geological Transactions), that the white oolite limestone at Hartlepool contains no shells or marine exuviæ; the authors found in it both univalves and bivalves, especially the latter." Now every geologist knows, that one part of a stratum may contain, and another be devoid, of organic remains; and that there were none in the quarry at Hartlepool, when I inspected it, I am certain, for specimens of the rock taken at the time are still in my possession. The next is in the letter press at p. 171, under the head of Dykes: "In a quarry at Whitley, near Cullercoats, where there is an extensive mass of magnesian limestone, detached from the great beds of the county of Durham, there is a similar dyke (see Geol. Trans. vol. iv. p. 25), which intersects the coal and sandstone strata under the limestone, and does not pass through the latter, &c." In reply to this, allow me to observe, I never said the dyke at Whitley was a basaltic dyke. My words at p. 25, are: "Besides the fissures filled with basalt, others of a very different nature intersect the coal field. These, if large, are called dykes, but if inconsiderable, troubles, slips, or hitches; and are the same that some geologists have called faults. I have already noticed the main, or 90 fathom dyke, when speaking of the limestone quarry at Whitley, &c. &c." And at p. 26, "It is highly probable it traverses the lead mine district, and produces lateral and valuable metalliferous veins therein." The word *basalt* never occurs in my description of this fissure. Letter press, p. 287, "In many instances, through a fondness for generalizing, or an attachment to theory, authors have bestowed the name of coal basin where it is by no means appropriate. Thus we read of the coal basin of Newcastle, or the coal basin of the Tyne and Wear, though the coal strata of that district are no more in the form of a basin than the metalliferous limestone on which they are understood to repose, or the magnesian limestone which reposes on them." To this I answer, that the numerous sections published in the Transactions of the Geological Society, prove the coal measures of this district to rise by gentle degrees towards north and south, and more rapidly to east and west; therefore, these strata must rest in a trough or basin, which by no means is the case with the encrinal limestone, or magnesian limestone beds. Note, p. 287, "Since the two preceding parts of the work were printed, we have seen a paper by Mr. W. entitled, 'Observations on the Eastern Part of Yorkshire, published in vol. v. of the Geological Society's Transactions. In that paper, Mr. W. states, that the coal formation which covers the shale forms a basin. Had that gentleman examined our district himself, instead of attempting to describe it from scraps of information collected from others, he might have avoided this mistake, as well as several other errors into which he has fallen." The observations in question were sent to the Geological Society so long ago as the year 1816, and extracts from them appeared in the *Annals of Philosophy* for that year, vol. viii. p. 140, and of course, I conclude, must have been known to the Rev. G. Young; and as that gentleman has more than once honoured me with a call since that period, had he hinted his suspicions of my never having investigated the part of the country where he now resides, I should have acquainted him that several years ago my affairs frequently called me not only to Whitby, but to Hull, Scarborough, Driffield, Stockton, &c.; and from notes taken on those occasions, together with colliery borings kindly communicated by Mr. Buddle, my paper, which, I am sorry to see appears to have given offence, was chiefly compiled.

I remain, Sir, your most obedient humble servant,

N. J. Winch.

Article IX.

A List of the Freshwater and Landshells occurring in the Environs of Bristol, with Observations. By J. S. Miller, ALS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Bristol, March* 11, 1822.

If the following catalogue of the fluviatile and terrestrial shells occurring in the neighbourhood of Bristol, drawn up from my own personal observations, should be judged worthy of a place in the *Annals of Philosophy*, it is entirely at your service.

Your obedient servant, J. S. Miller.

To attach any high degree of value to local catalogues of this nature would be to exaggerate the interest and importance of contributions which can hold but an humble and subordinate place in the pages of science, still they are far from useless. Being founded on personal observations more precise in proportion as they are more limited, they furnish the surest foundation on which more general results may be built, and in the present stage of zoological inquiry can hardly fail to afford many new facts.

Thus the field of observation which the present list comprises will be found to afford several species not hitherto recognised as British, and some hitherto entirely nondescript. I have been enabled also, in the course of my local researches, to glean some new facts with regard to the habits and organization of some of these animals which appear to have escaped the notice of former inquirers, and have availed myself of the present opportunity of recording them.

In the following list I have adopted the names published in Dr. Maton and Mr. Rackett's Descriptive Catalogue of British Testacea in the Linnean Transactions, vol. viii, and occasionally added those new generic names which have been stated by more modern authors.

1. Mya pictorum. Freshwater.
   Unio pictorum. Drapernaud.
2. Tellina cornea. In ditches.
   Cyclas cornea. Drapernaud.
3. Tellina lacustris. In ditches.
   Cyclas lacustris. Drapernaud.
4. Tellina pusilla. In pools. Dillwyn's Cat. of Rec. Shells.
5. Tellina amnica. In ditches.

Observation.—I have met with individuals of the above four species of tellina, containing minute young living shells which prove the animals viviparous.

6. Mytilus anatina. In rivers and pools.
   Anodanta anatina. Drapernaud.

Observation.—I perfectly agree with Dr. Maton in considering M. avanensis only a variety of M. anatina. Miss Bennett, of Nortonhouse, favoured me lately with specimens from Tisbury, Wiltshire. They are old shells, and from the animal having lived in water, highly impregnated with chalk and calcareous matter, its epidermis has secreted so rapidly, and increased the shell so much in thickness, that the Linnean character "testa fragilissima" is entirely lost.

7. Bulla hypnorum. In ditches.
   Physa hypnorum. Drapernaud.
8. Bulla fontinalis. In ditches.
   Physa fontinalis. Drapernaud.
9. Voluta denticulata. In the Avon below the Hotwells.

Observation.—Its columella does not continue to the commencement of the spire, which is empty, and shows no spiral septa.

10. Buccinum terrestre. On Leigh and Clifton Down.
    Bulimus acicula. Drapernaud.
11. Turbo elegans. In Leighwood, &c.
    Cyclostoma elegans. Drapernaud.
12. Turbo fontinalis. In ditches.
    Cyclostoma obtusum. Drapernaud.
13. Turbo nautileus. In ditches.
    Planorbis cristatus. Drapernaud.
14. Planorbis imbricatus. Drapernaud. In ditches and pools.
15. Turbo cristatus. In ditches.
    Valvata sperorbis. Drapernaud.
16. Valvata minuta. Drapernaud.

Observation.—Of this, I have only found two dead shells in the drifted sand, &c. on the Banks of the Avon.

17. Turbo laminatus. Leighwood.
18. Turbo nigricans. Leighwood.
19. Turbo Everetti (*nobis*), on willow trees, near river banks.

Spec. Char. A turretted, fusiform, ventricose striated, brown, opaque shell, with nine reversed volutions. Aperture with two teeth.

Observation.—I consider this a distinct species, it having only nine volutions, whereas T. nigricans has always twelve. I have named it after W. Everett, Esq. of London, a gentleman zealous in the study of British conchology.

Obser. a.—Montagu states the frequent occurrence of turbo laminatus and nigricans deprived of their brown epidermis. But I have suites of specimens of all the three species in various stages of growth, which have a white epidermis, show no mark of being worn, and are evidently interesting, though rare varieties.

Obser. b.—As I frequently make sections of shells by grinding them down to come at the details of their internal formation, I discovered, in the year 1814, the valve-like appendage which turbo laminatus and nigricans possess. When Dr. Leach visited me in 1815, I pointed it out to him, which he considered as a new and interesting discovery. The Doctor furnished me with several other reversed shells to facilitate my further researches on this peculiar organization, and informed me subsequently (when in Paris) that M. Drapernaud had noticed the valve-like appen dage, and from it derived in part the character of his new genus Clausilia. This information deterred me at the time from further inquiry; yet as I have subsequently found that M. Drapernaud seems at a loss concerning the use of this valve; and M. Cuvier, in his Regne Animal, vol. ii. p. 409, states, "de cette petite lame, on ignore l'usage dans l'animal vivant;" I will here venture to add the opinion which I have myself been led to form on this subject.

Independently of the various contrivances which Nature has resorted to for the protection of the otherwise easily vulnerable molluscæ, it has taken peculiar care to guard the apertures of many univalves from the intrusion of enemies. Hence the apertures are sometimes peculiarly contracted, and provided with numerous folds and teeth. Other molluscæ have a calcareous operculum, permanently affixed, which increases in thickness, and enlarges on a depressed spiral plane, as the opening of the shell extends with the growth of the animal thus continually assimilating to its size, and when the animal retreats, excluding it completely from all external intrusion. In the clausilia, Nature has combined the protection afforded by means of contraction and folds, and also added an opercular appendage. The inhabitants of the Clausilia, when nearly full grown, secretes a thread-like, elastic, calcareous filament, one of whose ends is affixed to the columella. This filament makes a half spiral turn round the columella insinuating between its folds. When the animal finishes its shell, and completes the aperture, it secretes at the unattached end of the filament a spoon-shaped calcareous lamina conforming at its margin to the contour of the aperture. The lamina is somewhat smaller than this, and its margin is rounded. Its adhesion to an elastic filament enables the animal to push it when it comes out of its shell against the columella, and the same elasticity closes it, on the inhabitant retreating, thus securing it from intruding enemies. Thus then this valve may be compared to a door provided with an elastic spring. The elasticity of the filament may be restored to its full power by some times immersing it in water, as I have ascertained in sections made with a view to this inquiry.

20. Turbo juniperi. On mountain limestone rocks.
21. Turbo muscorum. In moss.
22. Turbo sexdentatus. Leighdown.

Observation.—The above three species belong to Drapernaud's genus Pupa.

23. Turbo carychium. In moss.
Auricula minima. Drapernaud.
Carychium myosotis. Ferussac.

Observation.—Why does M. Cuvier place this shell, whose animal lives in moss, among those inhabiting freshwater?

24. Helix lapicida. Leighwood.
25. Helix planorbis. In ditches.
26. Helix vortex. In ditches.
Planorbis vortex. Drapernaud.
27. Helix spirorbis. In ditches.
28. Helix contorta. In ditches.
Planorbis contortus. Drapernaud.
29. Helix alba. In ditches.
Planorbis hispidus. Drapernaud.
30. Helix fontana. In ditches.
31. Helix paludosa. In moss.
32. Helix ericetorum. Clifton Down.
33. Helix virgata. In fields.

Observation.—The abundance of this species in a field at Torkington, a few months ago, occasioned the report, that it had rained snails!

34. Helix caperata. In dry situations.
35. Helix rufescens. In hedges, &c.
36. Helix nitens. Under stones in moist places.
37. Helix alliaria (*nobis*).

Spec. Char. An umbilicated, depressed, pellucid, shining horn-coloured shell, having no more than four volutions.

Observation.—This species never arrives to the size of H. nitens, has one volution less, and is found under moss on old trees. Its inhabitant smells strongly of garlick.

38. Helix cristallina. Muller Verm.

Observation.—Found near the roots of grass, resembling the two former species; but infinitely smaller.

39. Helix cantiana. Near hedges.
40. Helix hispida. Leighwood.
41. Helix radiata. On old trees.
Helix rotundata. Drapernaud.
42. Helix umbilicata. In dry rocky situations.
43. Helix subrufescens (*nobis*). In woods.

Spec. Char. A subumbilicated, very slightly carinated, irregularly striated, slightly raised, diaphanous shell, with five volutions, and a somewhat round lunated aperture.

Observation.—I have found but few individuals of this new species, which differ from H. rufescens in the shell being thinner, rather corneous, and but very slightly carinated. From H. hispida, it differs in being only subumbilicated, and not hispid.

44. Helix pomatia. Rare at Stapleton.
45. Helix arbustorum. In woods.

46. Helix nemoralis. In hedges, &c.

47. Helix hortensis. In hedges, &c.
Helix grisea. Dillwyn.

Observation.—Of this, a more turreted variety occurs in some places.

48. Helix lackhamensis. In woods.
Bulimus montanus. Drapernaud.

49. Helix obscura. In woods.
Bulimus obscurus. Drapernaud.

50. Helix lubrica. In moist places.
Bulimus lubricus. Drapernaud.

51. Helix palustris. In pools.
Limneus palustris. Drapernaud.

52. Helix fossaria. In ditches.
Limneus minutus. Drapernaud.

53. Helix succinea. In moist places.
Succinea amphibia. Drapernaud.

54. Helix putris. In ditches, &c.

55. Helix tentaculata. In ditches.
Cyclostoma impurum. Drapernaud.

56. Helix auricula. In the Froom.
Limneus auricularius. Drapernaud.

57. Testacella Maugii. Sowerby.

Observation.—Mr. T. Drummond, jun. while engaged at Messrs. Sweet's and Miller's Nursery, informed me, in 1816, of the occurrence of a limax in their grounds, with a minute shell at its tail. This proved to be a testacella, and has been lately described by Mr. G. B. Sowerby as a new species in his recent publication on the Genera of Recent and Fossil Shells. It probably was introduced into that Nursery with foreign plants, but propagates now freely in the open ground; bears the winter, and increases much in rich soil; so that it can no longer be considered as an alien. I have sent from time to time a great many specimens to my scientific friends; so that I believe they are now pretty much distributed, and in the collections of many British conchologists. The animal lives on earth worms, which it draws in, with its proboscis-like mouth, entire; and if taken hold of, when thus gorged, disgorges it immediately. The earth worms frequently swallow the young testacella, and we may sometimes meet the shells in their intestines. The testacella lays but few eggs; these are ovate, and if placed on the hand, frequently burst like a soap-bubble, dispersing in minute fragments.

58. Nerita fluviatilis. In pools near the Avon.

59. Patella lacustris. In pools and ditches.
Ancylus lacustris. Drapernaud.

60. Patella oblonga. In the river Froom.
Ancylus fluviatilis. Drapernaud.

I cannot close this list without mentioning an undescribed helix found by me in 1817 on the boards that line a pine (bromelia) bed.

Helix Goodallii (*nobis*).

Spec. Char. A subperforated, turretted, pellucid, pale corneous, or almost white shell, having from six to seven volutions, and an ovate aperture.

Observation.—The inhabitant a limax of a green-yellowish colour, which is transmitted through the shell, and gives it that tinge when found with the animal in it. On account of the pine bed being frequently disturbed, full grown specimens are rare, and I possess but few that show seven volutions, the major part having from four to five. When full grown, one-third of an inch, or rather more, long. I have sent specimens of it (as a new bulimus to which modern genus it belongs) to the Linnean Society, Mr. Sowerby, Dr. Goodall, and several other gentlemen. I have named it after Dr. Goodall, the Provost of Eaton, so well known as a conchologist, and who had the goodness to communicate it to Baron de Ferrusac, at Paris, now engaged in publishing a splendid work on Land and Freshwater Molluscæ.

ARTICLE X.

Remarks on Mr. Moyle's "Observations on the Temperature of Mines in Cornwall." By R. W. Fox, Esq.

(To the Editor of the *Annals of Philosophy.*)

ESTEEMED FRIEND, *April* 20, 1822.

In 1819 and the two following years, I made some communications to the Cornwall Geological Society on the temperature of several mines in this county, to which subject my attention had been directed in 1815; and some of the results noticed therein had been obtained in that year.

A friend of mine, who had assisted me in my inquiries, being about to visit France, I communicated to him the substance of my papers with a view of obtaining information respecting the temperature of the mines in that country; and through this channel, some of the facts mentioned in them were introduced into the Annales de Chimie et de Physique.

I observe that the last number of the *Annals of Philosophy* contains a letter from M. P. Moyle, alleging, "that either I have drawn false conclusions on this subject, or that the temperatures have not been taken in a proper manner."

As the second volume of the Transactions of the Cornwall Geological Society, in which, I understand, my communications are to be inserted, is now in the press, I shall at present refrain from entering into a detail of the facts which they contain. I must, however, observe, that my conclusions have been drawn not only from the temperature of the veins, but from that of cross levels at a considerable distance from them, and of those parts of the mines which were least affected by currents of air, and in which there were few or no workmen; and although I am well aware that many adventitious, and indeed opposite, causes operate in mines, which render it difficult to obtain satisfactory data as to the true temperature of the earth at equal depths, I think it will appear, when the above-mentioned volume is published, that due precautions have not been neglected to prevent their effects as much as seemed practicable.

The temperature at different depths and stations in 13 mines, which varied from 540 to 1430 feet in depth, and averaged above 800 feet (being all those from which I have received any information) is given in the communications to which I have referred; and not one instance has occurred in the course of my inquiries in which the temperature was not greater in the deepest part of the mine than near the surface; and in most cases, it increased in proportion to the depth. This remark applies whether the temperature of the air, of the solid ground, or of the jets of water, as they flowed into the mine, were taken; yet commonly, a very small proportion of the workmen in deep mines are employed in the lowest galleries.

I think the following facts are sufficient to prove, that the heat, in some mines at least, must be attributed to some other cause than the presence of the workmen, &c. An opportunity occurred some time ago at Treskerby Mine, which is 840 feet deep, to ascertain the temperature after the workmen had been absent for two successive days; when it was found that no diminution of heat had taken place during that time; but that both the water (which flowed copiously into the bottom of the mine) and the air continued at 76°.

At the end of the deepest gallery in Dolcoath Mine, 230 fathoms, or 1380 feet, under the surface, a thermometer four feet long was inserted to the depth of three feet in the ground, and was closed round with earth. In this situation, it was left more than eight months, during which time no workmen were employed near it; it was frequently examined, and it denoted a constant temperature of 75° or 75½°, except when the water from accidents to the machinery gained on the pumps, and filled the gallery, which occurred more than once for some weeks together. Immediately after the water had been drawn out, the mercury was found to have risen to 76° or 77°; but in a few days, it resumed its previous station of 75½°.

An increase of temperature was also produced in the United Mines in the two deepest galleries, 1140 and 1200 feet under the surface, in consequence of an influx of water for a few days;

in the former, it was 87·5°, and in the latter 88°, which is by much the highest temperature I have heard of in any of the mines of this county.

For a statement of the other facts I have collected, and also of the temperature of cross galleries, I must beg to refer to the Transactions of the Cornwall Geological Society; but I may remark, that the latter were generally a few degrees under the temperature of the galleries in the direction of the metallic veins at the same level; and this small difference does not appear extraordinary, when it is considered that independently of the latter being sometimes more affected by extraneous causes, the veins afford an easier passage to the water and vapour, than the more compact ground in which they are enclosed.

As far as my inquiries have gone, I consider the ratio of the increase of temperature may fairly be estimated at about one degree for every 60 or 70 feet in depth.

M. P. Moyle states, that he found a gallery in Huel Unity, 150 fathoms deep, of the temperature of 65°; this being 12° above the mean temperature of our climate, taking it at 53°, as I have done (which is, I believe, rather too high than too low an estimate), nearly approximates to the ratio of increase before mentioned.

In the case of Huel Trevenen Tin Mine, he describes the temperature of the water to have been lower than that of the atmosphere; but as he does not say what the temperature of either was, no inference, I conceive, can be drawn from this case. And here I may remark, that in Tincroft, and Cook's Kitchen Mines, which had been for a long period partly full of water, the temperature was found to increase considerably in descending; although in a less ratio than in other mines which were not so circumstanced; and this I attribute to the influence of evaporation, and the accumulation of colder water from the surface.

Huel Trumpet Tin Mine appears to present the only exception which M. P. Moyle has specified. Not having visited the mine, I am ignorant of the circumstances of the case; and whether it be a copious stream from the vein, or only some water accumulated by dropping from superior strata, which is reported to have been at 51°, we are not informed: if, however, it be an exception, let not the cold stream of Huel Trumpet be a stronger argument on one side of the question, than the hot spring of Iceland is admitted to be on the other.

Respectfully thy friend,
R. W. Fox.

Article XI.

An Answer to Mr. Murray's "Reply." By B. M.*

(To the Editor of the *Annals of Philosophy.*)

SIR,

Mr. Murray is of opinion "that it is neither expedient nor profitable to exchange thrusts with a shadow." I admit the propriety of this as a general rule, trusting you will for once allow me to violate it by replying to Mr. Murray.

This gentleman informs us, that the experiments which I have already asserted and maintain to be fallacious, "comprise only a *very few* selected from a very many on the subject in question, and he drew his inferences from the combined aggregate, and not from individual or insulated phænomena."

The only meaning which I can discover in this passage is, that although a part of a number of experiments may be inaccurate, yet as they are accompanied by others, which may be equally fallacious, inferences may be fairly deduced from the whole.

Mr. Murray has recommended magnetized steel filings in cases of poisoning by corrosive sublimate, on the supposition that they are more efficacious than those which are unmagnetized. This idea is grounded on the assumption that unmagnetized steel is incapable of effecting the decomposition of the muriate of mercury. I have shown this idea to be incorrect, and quoted various authorities and experiments to prove that steel is capable of decomposing muriate of mercury without being magnetized.

How has Mr. Murray answered this objection? Why, by stating that he "was not ignorant of the action of muriate of mercury or nitrate of silver on steel which B. M. had presumed to suppose."

I will repeat the grounds on which I rested my opinion of Mr. Murray's ignorance of these facts. I allow they are not good, but they are his own experiments. He says that "a solution of permuriate of mercury was by the magnet soon reduced into running mercury." Mr. Murray does not indeed here say that common steel is incapable of producing this effect, but he evidently supposes it by stating that he employed a magnet.

With respect to the action of steel upon nitrate of silver, the evidence as to Mr. Murray's knowledge of the subject is complete. He states that "fine Dutch steel wire was selected, and proved to be non-magnetic. It was thrown into nitrate of silver, where it remained for 14 hours without being affected. Part of this was made the connecting wire between the north and south poles of two bar magnets, when it became speedily plumed with crystals of silver."

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* See *Annals of Philosophy*, present volume, p. 41 and 121.

Now I do assert that if Mr. Murray believed in the accuracy of his own experiments, he was totally ignorant of the action of steel upon nitrate of silver; for in this experiment he has stated, and attempted to prove, that no action takes place. The experiment is indeed fallacious, but then it proves even more than Mr. Murray's ignorance of the facts with which he asserts that he was acquainted.

I had intended to offer a few more observations upon some parts of Mr. Murray's reply which I understand, and quoted others that I do not comprehend; but, I think, I have done enough. In parting with him, I would advise him in future, should his experiments excite any further notice, not to employ, in his reply, such terms as "rude" and "ungentlemanly;" they are harmless, except to the reputation of him who uses them.

I am, Sir, your obedient servant,

B. M.

Article XII.

Remarks on the Influence of Moisture in modifying the Specific Gravity of Gases. By James Apjohn, MD.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Trinity College, Dublin, April 20, 1822.*

Upon reading (in the number of the *Annals* for this month), Dr. Thomson's Observations on the Specific Gravities of the Gases as modified by Moisture, it at first struck me that some mistake had been committed in attributing to steam at 212° only the specific gravity of ·472. Into this opinion I had been led by recollecting that Dr. Ure, in his Dictionary of Chemistry, had stated it so high as ·625. A closer examination, however, proved that the specific gravity given it by Dr. Ure was too great, and indicated, as the probable source of the error, the assumption of air at 212° as the unit of comparison. Dr. Thomson has not overlooked this circumstance in his paper, but his mode of estimating the effect of moisture on the densities of the gases appears to me altogether incorrect. The principle of the method adopted by Dr. Ure for the same purpose is true, but his number for steam being too high, his results are erroneous. As this is a subject of some importance, I trust I shall be excused for entering a little into detail. It has been shown by Dalton and Gay-Lussac, that $\frac{px}{p-f}$ will represent the volume which any given bulk of dry gas, as x, assumes, when saturated with steam whose elasticity is f, p representing the atmospheric pressure at the time. Let the increased volume equal v, and we shall have $x = v \times \frac{p-f}{p}$, and the expansion produced by the steam $= v - v \times \frac{p-f}{p} = v \times \frac{f}{p}$. The ratio, therefore, of the dry gas to the expansion produced in it by the moisture is that of $\frac{p-f}{f} : 1$. Dr. Thomson, through some oversight, considers this as the ratio in volume of the dry gas to the steam with which it is saturated, whereas the true ratio of these is that of $\frac{p-f}{p} : 1$, inasmuch as a given volume of gas at any temperature saturated with moisture contains as much steam as could exist at that temperature in a vacuum of equal capacity. The following, therefore, is the true expression for the specific gravity of a gas saturated with moisture. Calling it y, $y = \frac{p-f}{p} \times a + b$, a and b denoting the respective specific gravities of the dry gas, and of steam whose elasticity is f. The following table, constructed from this formula, exhibits the specific gravities of some of the principal gases when saturated with moisture at the temperature of 60°. In calculating b, the specific gravity of steam at 60°, I have used Mr. Dalton's table of elasticities.

Gas.	Sp. gr. of dry.	Sp. gr. of moist.
Air	1·0000	0·9907
Oxygen	1·1111	1·0998
Nitrogen	0·9722	0·9634
Chlorine	2·5000	2·4644
Hydrogen	0·0694	0·0761

The expression given above may easily be made to assume the form of $y = a - \frac{fa}{p} + b$. Then, by substituting for b its value, which is $\cdot 472 \times \frac{f}{p}$, we shall have $y = a - \frac{fa}{p} + \cdot 472 \times \frac{f}{p}$. From which it appears that the specific gravity of the moist gas is equal to, less, or greater than that of the dry, according as the specific gravity of the dry gas is equal to, *greater*, or *less* than ·472, the specific gravity of steam at 212°. This result is worth recollecting, as by it the subject of moisture, as modifying gaseous specific gravity, is divested of all perplexity. Thus by it we learn, that hydrogen is the only one of the permanent gases whose density, at least at all ordinary temperatures, can be increased by moisture, as it is the only one whose specific gravity is less than ·472. The densities of all the rest are diminished by moisture, and that, the more as we descend in the scale of temperature; for as we descend, their specific gravities increase, and of course the differences between them and the constant quantity ·472. As an inference also from this result, I may remark, that the rule so much insisted upon, of taking the specific gravities of the gases at a low temperature, is so far from being general, as to apply to hydrogen alone. In order to determine the specific gravity of a gas, it is only necessary, as is well known, to take the weights of equal bulks of it and atmospheric air at the same temperature, the former divided by the latter, giving the specific gravity required. Air, however, is always impregnated with more or less moisture, and the other gases, as usually collected, are saturated with it. Unless, therefore, they be, previous to weighing, thoroughly dried, the resulting number must be inaccurate. Dr. Thomson's mode of reducing the error, which consists in saturating the gas and air before weighing them, is valuable for all the gases but hydrogen. In its case, the error would be thus increased instead of diminished. The following mode of eliminating the error is perfect, and sufficiently simple. Let the given gas and the air be both saturated with moisture. Then if W = weight of moist air, and W' = weight of moist gas, c = capacity of the flask in cubic inches, and b = specific gravity of steam at the common temperature of the gas and air, $\frac{W' - \cdot 305\, b\, c}{W - \cdot 305\, b\, c}$ will express the specific gravity of the gas in its dry state: $\cdot 305\, b\, c$ = weight in grains of a volume of steam whose magnitude = c, and specific gravity = b. The rationale is obvious.

ARTICLE XIII.

ANALYSES OF BOOKS.

The Use of the Blowpipe in Chemical Analysis, and the Examination of Minerals. By J. J. Berzelius, Member of the Academy of Sciences at Stockholm, &c. Translated from the French of M. Fresnel, by J. G. Children, FRS. L. & E. FLS. MGS. &c. *With a Sketch of Berzelius's System of Mineralogy; a Synoptical Table of the principal Characters of the Pure Earths and Metallic Oxides before the Blowpipe, and numerous Notes and Additions, by the Translator.* London, 1822.

We have great satisfaction in announcing the appearance of this translation of Berzelius's work on the Blowpipe. When the number of minerals is almost daily increasing, and when every discoverer of a new locality is giving a fresh name to a mineral which to him may be new, although others may be well acquainted with it, a work, like the present, must be deemed of great importance, as enabling mineralogists to decide upon the nature of a specimen without having recourse to a tedious, and frequently unsuccessful, analysis.

No person could be selected from among the numerous philosophers of which the present day has to boast, who is better calculated than Berzelius for the task which he has undertaken. We fully concur in the statement of the translator, "that the name of Berzelius, as a skilful and patient experimenter, stands almost unrivalled;" and that the present essay, although occasionally obscured and perplexed by his peculiar hypothetical notions, "amply vindicates his claim to the high reputation he has acquired."

The use of the blowpipe cannot be more clearly or better described than in the author's introduction. "In the analysis of inorganic substances, the use of the blowpipe," he observes, "is indispensable. By means of this instrument, we can subject portions of matter, too small to be weighed, to all the trials necessary to demonstrate their nature, and it frequently even detects the presence of substances not sought for nor expected in the body under examination. The facility that it affords for discovering the constituent parts of metallic fossils, renders it equally indispensable for the miner, whose common processes are sometimes singularly disturbed by the occurrence of foreign substances in the minerals he operates on, and whose nature, for want of time or skill, he can but seldom ascertain by sufficiently elaborate and delicate chemical experiments, but which the ready and convenient use of the blowpipe enables him to develope in a few seconds. To the mineralogist, this instrument is absolutely necessary, as his only resource for immediately ascertaining if the inference he draws from external characters, such as form, colour, hardness, &c. be legitimate."

With respect to his work, Berzelius remarks, that it is "a system of chemical experiments, made in the dry way, as it used to be called, and almost always on a microscopic scale, but which presents us in an instant with a decisive result." He has evidently been at great pains in selecting the specimens upon which he operated, having been supplied with them by Haüy, Bournon, Gillet de Laumont, Brongniart, Brochant, and other names well-known to mineralogists of every country.

In his historical sketch of the blowpipe, Berzelius refers to Gahn, who, he assures us, attained to such a degree of skill in its use, that he could detect the presence of substances in a body by its means, which had escaped the most careful analyses, conducted in the moist way. "Thus," says Berzelius, "when Ekeberg asked his opinion respecting the oxide of columbium, then recently discovered, and of which he sent him a small specimen, Gahn immediately found that it contained tin, although that metal does not exceed 1-100th of the weight of the mineral." To this he adds, that long before the question was started, whether the ashes of vegetables contain copper, "I have seen him many times extract, with the blowpipe, from a quarter of a sheet of burnt paper, distinct particles of metallic copper."

These facts are worthy of particular notice, and they offer great encouragement to those who have hitherto neglected the use of the blowpipe, or have rejected it as difficult, to renew their attempts.

The parts of which the book, or rather the translation, consists, are: The translator's dedication to Sir H. Davy;—his preface and a note to the reader, explaining and rectifying some mistakes, induced by Berzelius's hypothetical notions of combination, and his attempt to reconcile the theory of volumes with the atomic or corpuscular theory. We have then a sketch of Berzelius's mineralogical arrangement by the translator. This is followed by the author's introduction, and the remainder, constituting of course the greater part of the work, is arranged under the following heads: Description of the blowpipe, including the flask, flame, and support; the reagents, and their use, follow; and of these a very complete account is given. We have then the pyrognostic characters of the alkalies, earths, and metallic oxides, detailed: these are followed by the characters of minerals; and the work concludes with an account of the phænomena developed by urinary calculi before the blowpipe.

It is to be observed that Mr. Children has introduced a very useful Synoptical Table of the principal characters of the pure earths and metallic oxides before the blowpipe.

It may not, perhaps, be uninteresting to the reader, to have an example of the mode in which mineral bodies are treated in this work: we give at hazard:

"*Phosphate of iron* in bluish transparent crystals, from St. Agnes, in Cornwall.

"*Alone, in the matrass,* gives a great deal of water, intumesces, and becomes sprinkled with grey and red spots.

"*On charcoal,* intumesces, reddens by the heat, and then very readily fuses into a steel-coloured globule, with a metallic lustre.

"*With borax and salt of phosphorus,* behaves like oxide of iron.

"*With soda,* on charcoal, in the reducing flame gives grains of iron, which are attractable by the magnet. On platina foil, there is no indication of manganese.

"*With boracic acid* dissolves readily, and by the addition of metallic iron, in the manner detailed at p. 129, gives a fused regulus of phosphuret of iron.

"All the varieties of phosphate of iron that I have had an opportunity of examining, behave in the same manner"

The number of reagents is not very great, and they are easily procured in a state of purity: they are carbonate of soda, borax, salt of phosphorus, prepared by dissolving together 16 parts of muriate of ammonia and 100 parts of crystallized phosphate of soda, and crystallizing the solution; vitrified boracic acid, nitre, gypsum, fluor spar, solution of nitrate of cobalt, tin, iron, bone ashes, silica, and oxide of copper.

Three plates accompany this work; two represent the instruments recommended by the author, and which may be procured either at Messrs. Knights', in Foster-lane, or Mr. Newman's, in Lisle-street. The other plate is introduced by the translator, representing Brooke's, or Newman's blowpipe, an account of which has been very properly introduced, although, we think, it is a very powerful rather than a very useful instrument.

Our observations have been hitherto nearly confined to the original work; but we should do even that injustice without noticing the share which the translator has had in forwarding and elucidating the views of his author. Mr. Children is too well known to require any encomium from us for the zeal which he has manifested in every thing relating to chemical science. His acquaintance with the blowpipe has already been exhibited in his translation of the fourth volume of Thenard's Chemistry; but both on that and the present occasion, we must consider him rather as the illustrator than the mere translator.

In our opinion, he has most properly rejected Berzelius's signs and formulæ; and connected as they are with their author's peculiar views of atomic composition, we think, with Mr. Children, "that they are rather calculated to perplex than facilitate our progress." That the reader, however, may not lose the information they are intended to convey, Mr. Children has subjoined in notes, the compounds they respectively indicate in common language. The least useful part of the translator's labour has, we think, been the introduction of a sketch of Berzelius's mineralogical arrangement. It will, however, probably have its use by deterring others from hastily following him in similar attempts. Who, for example, is likely (in this country at least) to describe a garnet as a *bisilicate of protoxide of iron, silicate of protoxide of manganese, and silicate of alumina?*

Notwithstanding this last remark, we beg most earnestly to recommend the work to our readers; and to offer both to the author and translator our best thanks for the benefit which they have bestowed both on chemical and mineralogical science.

ARTICLE XIV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

March 28.—On the Anatomy of Whales. By W. Scoresby, Esq.

April 18.—On the Changes that have taken Place in the Declination of some of the principal Fixed Stars. By John Pond, Esq.

Extract of a Letter from Capt. Sabine to the President.

Some Observations on the Buffy Coat of the Blood.

April 25.—On the Nerves. By Charles Bell, Esq.

ARTICLE XV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Arrow Root.*

Indian arrow root grows wild in every part of the island of St. Michael. At present, it is almost entirely neglected by the natives, but some of the foreign families prepare small quantities of it for their private use. The root in its natural state is extremely acrid to the taste, and if chewed, produces a profuse salivation; when applied to the skin for some time, it produces heat, redness, and pain. The preparation consists in separating the fecula by careful and repeated washings after the root has been grated; but the effects produced by handling the root, are so unpleasant, that persons can with difficulty be hired to conduct the necessary operations.—(Dr. Webster's Description of the Island of St. Michael.)

II. *Seeds of the Croton Tiglium.*

It appears, from the experiments of Dr. Nimmo, that these seeds which yield the very active oil of croton, lately introduced as a purgative, consist of,

Acrid matter soluble in alcohol.....	27·5
Fixed oil soluble in oil of turpentine.	32·5
Farinaceous matter insoluble in both.	40
	100·0

Digesting sulphuric ether upon 100 parts of the bruised seeds, throwing the whole upon a filter, covering it closely during the process of filtration, and washing the residuum with a sufficient quantity of ether, it was found to weigh 40 parts, 60 having been dissolved.

By this process, from 300 grains of the seeds, from which, if 102 grains are deducted for the shells, there are left 198 grains of the kernels, there were obtained upwards of two drams, by measure, of an oil which possessed all the qualities, as to taste and medicinal efficacy, which the purchased oil contained.—(Journal of Science, Literature, and the Arts.)

III. *Specific Gravities.*

The following specific gravities have been taken by M. M. Roger and Dumas, with great accuracy.

	Specific Gravity.
Ice	0·950
Silica	2·650
Boracic acid	1·830
Arsenious acid	3·698
Protoxide of copper	5·749
Oxide of bismuth	8·449
Oxide of lead	8·010
Peroxide of mercury	11·29
Caustic lime	3·08
Carbonate of lime	2·717
Anhydrous sulphate of lime	2·960
Crystallized sulphate of lime	2·322
Alumina	4·200
Nepheline	3·270
Sulphur	2·086

(Edin. Phil. Journal.)

IV. *Effect of Heat on the colouring Matter of the Ruby.*

In subjecting rubies to high degrees of heat, Dr. Brewster observed a very singular effect produced during their cooling. At a high temperature the red ruby becomes green: as the cooling advances, this green tint gradually fades, and becomes brown, and the redness of this brown tint gradually increases till the mineral has recovered its primitive brilliant red colour. A green ruby suffered no change from heat; and a bluish green sapphire became much paler at a high heat, but resumed its original colour by cooling.—(Edin. Phil. Journal.)

V. *Large Human Calculus.*

The Reverend J. Cumming, Professor of Chemistry, at Cambridge, has lately given an account to the Philosophical Society, of a calculus in the possession of Trinity College, which weighs 32 ounces; its specific gravity is 1·756, and it measures 15½ inches in circumference. Its nucleus is lithic; to this succeeds a considerable portion of the oxalate of lime variety, followed by layers of the triple crystals, covered by a thick coating of lithic, which is occasionally broken by a layer of the triple crystals, and the external surface is principally composed of the fusible calculus. Professor Cumming notices also a calculus composed of vegetable matter and the phosphates found in the intestines of a horse, which weighs 64 ounces, and measures 37 inches in circumference.

VI. *Arseniuretted Hydrogen Gas.*

M. Serrulas proposes the following method of preparing this gas. A mixture is to be made of two parts of antimony, two parts of bitartrate of potash, and one part of arsenious acid, they are to be well triturated together in a mortar, and heated strongly for two hours in a close crucible. The alloy which results, when in contact with water, produces hydrogen gas, saturated with arsenic, and it may be preserved for any length of time in close vessels: to obtain the gas, about 150 grains, reduced to coarse powder, are to be put quickly under a jar filled with water, and inverted in a glass basin containing water. Many cubic inches of arseniuretted hydrogen gas will be obtained in a few minutes.—(Journal de Physique.)

VII. *Analysis of the Roots of Black Hellebore.*

M. M. Feneulle and Capron have lately analysed the roots of black hellebore. The products which they obtained were:—1. A volatile oil; 2. A fatty matter; 3. A resinous matter; 4. Wax; 5. A volatile acid; 6. A bitter principle; 7. Mucus; 8. Alumina; 9. Gallate of potash, and acidulous gallate of lime; 10. A salt, with an ammoniacal base.

VIII. *Heavy Spar.*

Stromeyer has published an analysis of the heavy spar of Nutfield, in Surrey, from which it appears, that it contains no sulphate of strontian; and further, that the proportions of the earth and acid are nearly the same as in the artificial sulphate of barytes. This latter fact, Stromeyer remarks, is of importance, from its showing that the natural combinations of bodies are constituted according to the same fixed proportions as those which are formed artificially.—(Edin. Phil. Journal.)

It has surely been long known that the composition of artificial sulphates, carbonates, &c. is similar to the natural.—*Ed.*

IX. *Slide of Alpnach.*

M. Rupp, a skilful engineer, of Wirtemberg, constructed some years ago a slide for the purpose of conveying fir trees from the forest of Mont Pilate, near Lucerne, into the lake of that name. The distance which they had to be conveyed is about 46,000 feet. The medium height of the forest is about 2500 feet. The horizontal distance, when reduced to English measure, is about eight miles. The declivity is one foot in 17·68; the medium angle of elevation 3° 14′ 20″. This declivity, though so moderate on the whole, is in many places very rapid; at the beginning of the inclination it is about one-fourth of a right angle, or about 22° 30′, in many places it is 20°, but no where greater than 22° 30′.

Along this line the trees descend, in a sort of trough, built in a cradle form, and extending from the forest to the edge of the lake. Three trees squared and laid side by side form the bottom of the trough; the tree in the middle having its surface hollowed, so that a rill of water, received from distance to distance over the side of the trough, may be conveyed along the bottom and preserve it moist. The trees which descend by this conveyance are spruce firs, very straight, and of great size. All their branches are lopped off; they are stripped of the bark, and the surface made, of course, tolerably smooth. The tree is launched with the root foremost into the steep part of the trough, and in a few seconds acquires such a velocity as enables it to reach the lake in the short space of six minutes. The late Professor Playfair, from whose works this notice is taken, saw five trees come down; the greatest of them was a spruce fir a hundred feet long, four feet in diameter at the lower end, and one foot at the upper. The slide crosses in its way three great ravines, one at the height of 64 feet, another at the height of 108, and the third, where it goes along the face of a rock, at that of 157; in two places it is conveyed under ground.

X. *Preparation of Sulphuret of Mercury.*

Dr. Taddei recommends the following process for the preparation of this substance, as being one which effects the combination immediately, and in a more perfect manner than that generally employed. Put one part of sulphuret of potash into a mortar, with three or four parts of mercury; triturate together, adding a little water by degrees, until the whole is reduced to a homogeneous black paste; then add flowers of sulphur, in equal quantity to the mercury employed, and mix the whole by a short trituration. Then wash the sulphuret with repeated portions of water till all the alcaline sulphuret is removed. The sulphuret thus prepared is not of the black colour of that obtained by simple trituration. Dr. Taddei says, that the addition of a little sulphuret of potash to the mixture of sulphur and mercury, does not render a long trituration unnecessary, but that, proceeding as above, the substance is prepared instantly.—(Giornale di Fisica, v. iv. p. 12.)

Article XVI.

NEW SCIENTIFIC BOOKS

JUST PUBLISHED.

The Use of the Blowpipe, in Chemical Analyses, and in the Examination of Minerals. By J. J. Berzelius, Member of the Academy of Stockholm, &c. &c. and translated from the French of M. Fresnel, by J.G. Children, FRS. L. and E. FLS. &c. &c. With a Sketch of Berzelius's System of Mineralogy; a Synoptic Table of the principal Characters of the Pure Earths and Metallic Oxides before the Blowpipe; and numerous Notes and Additions by the Translator. With Three Plates. 8vo. 12*s*.

Conversations on Mineralogy. With Plates, engraved by Mr. and Miss Lowry from Original Drawings, comprising upwards of 400 Figures of Minerals, including 12 beautifully-coloured Specimens. 2 Vols. 12mo. 14*s*. Boards.

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Zoological Researches in the Island of Java, &c. &c. with Figures of Native Quadrupeds and Birds. By T. Horsfield, MD. &c. No. I. Royal 4to., 1*l.* 11*s.* 6*d.* each. To be completed in Eight Numbers.

Remarks upon Morbus Oryzeus. By R. Tytler, MD. MAS. In Two Parts. 8vo. 8*s.*

The Works of John Playfair, Esq. late Professor of Natural History in the University of Edinburgh: with a Memoir of the Author. 4 Vols. 8vo. 2*l.* 12*s.* 6*d.*

ARTICLE XVII.

NEW PATENTS.

W. Ravenscroft, of Serle-street, Lincoln's-inn, peruke-maker, for a forensic wig, the curls whereof are constructed on a principle to supersede the necessity of frizzing, curling, or using hard pomatum, and for forming the curls in a way not to be uncurled; and also for the tails of the wig not to require tying in dressing; and, further, the impossibility of any person untying them.—Jan. 14.

D. Loescham, Newman-street, Oxford-street, and J. Allwright, Little Newport-street, for an improved keyed musical instrument. Communicated to him by a foreigner.—Jan. 14.

A. Gordon, London, and D. Gordon, Edinburgh, Esqrs. for improvements and additions in the construction of lamps, and of compositions and materials to be burned in the lamps, and which may also be burned in other lamps.—Jan. 14.

D. Gordon, Edinburgh, Esq. for improvements and additions to steam-packets, and other vessels; part of which improvements are applicable to other naval and marine purposes.—Jan. 14.

A. Applegath, Duke-street, Lambeth, for improvements in printing machines.—Jan. 14.

J. Hague, Great Pearl-street, Spitalfields, for a method of making metallic pipes, tubes, or cylinders, by the application and arrangement in the apparatus of certain machinery and mechanical powers.—Jan. 29.

Sir W. Congreve, Bart.; for improved methods of multiplying facsimile impressions to any extent.—Jan. 29.

P. Ewart, Manchester; for a new method of making coffer-dams.—Jan. 29.

R. Bill, Newman-street; for an improved method of manufacturing metallic tubes, cylinders, cones, or of other forms, adapted to the construction of masts, yards, booms, bowsprits, casks, &c.—Feb. 5.

F. L. Talton, New Bond-street; for an astronomical instrument or watch, by which the time of the day, the progress of the celestial bodies, as well as carriages, horses, or other animals, may be correctly ascertained. Partly communicated to him by a foreigner.—Feb. 9.

Article XVIII.

Astronomical, Magnetical, and Meteorological Observations.

By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Astronomical Observations.

March 2.	Emersion of Jupiter's first satellite.	7h 12′ 27″	Mean Time at Bushey.
		7 13 48	Mean Time at Greenwich.

Magnetical Observations, 1822. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
March 1	8h 33′	24° 32′ 47″	1h 30′	24° 37′ 46″		
2	8 30	24 26 42	1 30	24 40 40		
3	8 30	24 31 30	— —	— — —		
4	8 28	24 30 28	1 38	24 37 13		
5	8 30	24 28 18	1 34	24 36 51		
6	8 34	24 27 17	1 30	24 37 08		
7	8 28	24 28 04	1 30	24 38 05		
8	8 32	24 27 06	1 30	24 36 55		
9	6 36	24 27 24	— —	— — —		
10	8 32	24 26 43	1 30	24 35 43		
11	8 35	24 27 24	1 40	24 34 20		
12	8 37	24 27 37	1 25	24 36 17		
13	8 40	24 31 30	1 26	24 38 11		
14	8 35	24 27 45	1 25	24 36 30		
15	8 35	24 30 19	1 25	24 36 22		
16	8 25	24 31 50	1 16	24 36 47		
17	8 32	24 27 36	1 25	24 35 24		
18	8 30	24 26 40	1 25	24 36 58		
19	8 35	24 27 54	1 25	24 35 07	6h 05′	24° 32′ 12″
20	8 30	24 26 29	1 27	24 36 02	6 15	24 31 30
21	8 35	24 28 26	1 35	24 37 55	6 25	24 27 42
22	8 30	24 24 35	1 25	24 35 09	6 18	24 25 38
23	8 30	24 25 53	1 30	24 36 49	6 20	24 28 48
24	8 30	24 26 26	1 25	24 35 11	6 20	24 28 39
25	8 35	24 25 23	— —	— — —	6 25	24 27 29
26	8 30	24 24 45	— —	— — —	— —	— — —
27	8 30	24 25 25	1 27	24 38 15	6 25	24 30 20
28	8 32	24 25 55	1 34	24 35 26	6 13	24 28 58
29	8 28	24 26 30	1 28	24 36 13	6 20	24 28 14
30	8 40	24 26 15	1 45	24 35 46	6 27	24 27 54
31	8 35	24 25 46	1 30	24 35 26	6 30	24 27 33
Mean for Month.	8 32	24 27 38	1 29	24 36 36	6 20	24 28 45

By comparing the mean of these observations with those of March, 1819, the period of the greatest western variation, the morning decrease is 5′ 40″, the noon 5′ 06″, and the evening 6′ 32″; mean of the three 5′ 46″, which is an annual diminution of 1′ 55″.

Meteorological Observations.

Month.	Time.	Bar.	Ther.	Hyg.	Wind.	Strength.	Weather.	Six's.	
March		Inches.						Least	Greatest
1	Morn..	29·786	35°	63°	SSE	Pleasant	Fine	32°	46·5
	Noon..	29·736	45	56	WSW	Fresh	Very fine		
	Even..	—	—	—	—	—	—		
2	Morn..	29·811	43	71	SSW	Moderate	Sm. rain	36	52
	Noon..	29·819	51	61	SSW	Fresh	Cloudy		
	Even..	—	—	—	—	—	—		
3	Morn..	29·814	43	85	SW	Fresh	Fog	40	54
	Noon..	29·780	54	55	SSW	Fresh	Very fine		
	Even..	—	—	—	—	—	—		
4	Morn.	29·614	41	80	SSW	Fresh	Cloudy	39	53
	Noon..	29·525	52	62	SSW	V. fresh	Fine		
	Even..	—	—	—	—	—	—		
5	Morn..	29·482	45	75	W	Moderate	Cloudy	42	56
	Noon..	29·505	49	54	WSW	V. fresh	Fine		
	Even..	—	—	—	—	—	—		
6	Morn..	29·026	51	84	SW by W	Hard	Sm. rain	45	52·5
	Noon..	29·067	52	65	W by S	Hard	Cloudy		
	Even..	—	—	—	—	—	—		
7	Morn..	29·100	41	65	W	V. fresh	Clear	39·5	48·5
	Noon..	28·935	43	64	WNW	Very hard	Showery		
	Even..	—	—	—	—	—	—		
8	Morn..	29·168	37	68	W by S	V. fresh	Cloudy	35	47·7
	Noon..	28·882	47	66	W by S	Hard	Showery		
	Even..	—	—	—	—	—	—		
9	Morn..	29·177	40	64	W	Fresh	Very fine	38·3	50·5
	Noon..	29·220	—	78	SW	Moderate	Rain		
	Even..	—	—	—	—	—	—		
10	Morn..	29·110	50	76	SW	Hard	Showery	46	56
	Noon..	29·087	55	53	W	Hard	Showery		
	Even..	—	—	—	—	—	—		
11	Morn..	29·288	41	58	W	Hard	Very fine	36	46
	Noon..	29·429	44	52	W by N	Hard	Showery		
	Even..	—	—	—	—	—	—		
12	Morn..	29·881	37	59	W	Light	Clear	32·5	48·7
	Noon..	29·889	47	53	WSW	Moderate	Very fine		
	Even..	—	—	—	—	—	—		
13	Morn..	29·718	39	63	SE by S	Fresh	Fine	35·5	54
	Noon..	29·593	50	48	SSE	Fresh	Fine		
	Even..	—	—	—	—	—	—		
14	Morn..	29·435	47	69	SSW	Fresh	Cloudy	43	54·7
	Noon..	29·500	54	66	SW	Light	Rain		
	Even..	—	—	—	—	—	—		
15	Morn..	29·735	44	52	NE by E	Light	Clear	38·5	55
	Noon..	29·753	58	47	Variable	Light	Clear		
	Even..	—	—	—	—	—	—		
16	Morn..	29·713	47	86	SW by S	Fresh	Fog	40	53·3
	Noon..	29·641	53	60	SSW	Hard	Cloudy		
	Even..	—	—	—	—	—	—		
17	Morn..	29·784	49	70	W by S	Fresh	Cloudy	47	52
	Noon..	29·765	50	72	S	Moderate	Fog, rain		
	Even..	—	—	—	—	—	—		
18	Morn..	29·770	45	65	NW by N	Fresh	Fine	45	52
	Non..	29·900	51	57	W	Moderate	Cloudy		
	Even..	—	—	—	—	—	—		

Month.	Time.	Bar.	Ther.	Hyg.	Wind.	Strength.	Weather.	Six's.	
March		Inches.						Least.	Great.
19	Morn..	29·766	51°	78°	WSW	Fresh	Cloudy	44°	58°
	Noon..	29·800	56	59	W	Fresh	Cloudy		
	Even..	—	—	—	—	—	—		
20	Morn..	29·839	51	76	WSW	Moderate	Cloudy	49·3	56·5
	Noon..	29·823	53	68	WSW	Moderate	Cloudy		
	Even..	29·790	52	65	W by N	Light	Fine		
21	Morn..	29·785	48	71	WSS	Moderate	Fog	45	57
	Noon..	29·718	54	64	SW	Fresh	Fine		
	Even..	29·655	52	63	W by S	Fresh	Fine		
22	Morn..	29·811	45	56	WNW	V. fresh	Clear	40	55
	Noon..	29·878	52	51	W	V. fresh	Fine		
	Even..	29·878	51	55	W	Fresh	Fine		
23	Morn..	29·749	39	72	WSW	Fresh	Fog	36	60·5
	Noon..	29·635	57	45	SW	Fresh	Fine		
	Even..	29·493	50	56	SSW	Fresh	Fine		
24	Morn..	29·102	46	75	WSW	Fresh	Sm. rain	42·5	51·3
	Noon..	29·117	48	55	W by N	Fresh	Fine		
	Even..	29·194	43	54	WNW	Hard	Hail		
25	Morn..	29·258	49	68	SW by W	V. fresh	Cloudy	34	45
	Noon..	29·218	42	68	W	V. fresh	Rain		
	Even..	29·633	42	68	WNW	Fresh	Fine		
26	Morn..	29·543	44	70	W	Hard	Cloudy	35	55
	Noon..	—	—	—	—	—	—		
	Even..	—	—	—	—	—	—		
27	Morn..	29·725	49	70	SW by S	Fresh	Cloudy	44	58
	Noon..	29·703	56	60	SSW	Fresh	Cloudy		
	Even..	29·659	51	62	SSW	Moderate	Clear		
28	Morn..	29·474	52	59	SSW	Moderate	Fine	46	66
	Noon..	29·469	55	51	W by S	Fresh	Fine		
	Even..	29·500	56	57	W by N	V. fresh	Rain		
29	Morn..	29·907	47	52	W	Fresh	Very fine	41	56
	Noon..	29·946	56	45	W	Fresh	Fine		
	Even..	29·900	51	57	SW	Fresh	Fine		
30	Morn..	29·189	49	82	WSW	Hard	Rain	45	55
	Noon..	28·983	49	66	W	Squally	Showery		
	Even..	29·235	42	60	N by W	Hard	Showery		
31	Morn..	29·820	38	61	NNW	Hard	Fine	34	43
	Noon..	29·882	43	55	N	Fresh	Snow		
	Even..	29·930	38	60	N	Fresh	Cloudy		

Rain fallen between the 1st of March and noon the 1st of April, 0·715 inches. Evaporation, during the same period, 3·78 inches.

ARTICLE XIX.

METEOROLOGICAL TABLE.

1822.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
3d Mon.								
March 1	Var.	30·36	30·30	50	29	—		14°
2	S W	30·38	30·36	57	32	—		13
3	S W	30·38	30·19	58	28	—		
4	S W	30·19	30·07	58	39	—	11	16
5	N W	30·07	29·65	55	43	—	02	20
6	S W	29·69	29·65	55	41	—	10	2
7	W	29·75	29·56	55	34	58	11	4
8	S W	29·78	29·56	48	39	—	41	3
9	S W	29·85	29·72	52	47	—	05	2
10	S W	29·90	29·85	55	35	—	27	
11	N W	30·45	29·90	48	29	—		18
12	S W	30·45	30·28	50	31	—		16
13	S E	30·28	30·04	55	39	—		22
14	S W	30·29	30·04	60	31	53	05	3
15	N	30·29	30·25	53	37	—		
16	S W	30·33	30·25	57	50	—	01	10
17	S W	30·33	30·30	54	47	—	06	
18	N W	30·42	30·32	57	40	—		9
19	N W	30·35	30·32	62	50	—		3
20	N W	30·35	30·31	57	41	—		3
21	W	30·36	30·35	60	39	49		8
22	N W	30·39	30·28	57	37	—		17
23	W	30·28	29·70	66	44	—		23
24	W	29·86	29·28	53	36	—	—	
25	W	30·13	29·84	54	34	—	23	8
26	W	30·28	30·13	54	46	—		
27	S W	30·28	30·04	60	40	57		10
28	S W	30·45	30·01	72	46	—	—	18
29	S W	30·45	29·77	62	46	—		20
30	S W	30·39	29·58	58	33	—	18	11
31	N	30·45	30·39	48	32	48		
		30·45	29·28	72	29	2·65	1·60	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Third Month.—1. Hoar frost: fine. 2. Fine. 3. Hoar frost: fine. 4. Ditto. 5. Cloudy: very boisterous night. 6, 7. Showery. 8. Rainy. 9. Drizly. 10. Windy: rainy. 11, 12. Fine. 13. *Cirrocumulus.* 14. Drizzly. 15. Fine. 16. Drizzling. 17. Cloudy. 18. Fine: an ignis fatuus seen in the marshes near Bromley in the evening. 19—23. Fine. 24. Cloudy. 25. Rainy. 26. Cloudy. 27. Fine. 28. Fine, and very warm: *Cirrocumulus* and *Cirrostratus* during the whole of the day. 29. Fine. 30. Rainy. 31. Windy: cloudy.

RESULTS.

Winds: N, 2; SE, 1; SW, 15; W, 6; NW, 6; Var. 1.

Barometer:	Mean height	
	For the month	30·120 inches.
	For the lunar period, ending the 15th	30·213
	For 13 days, ending the 5th (moon north)	30·328
	For 15 days, ending the 20th (moon south)	30·095
Thermometer:	Mean height	
	For the month	47·338°
	For the lunar period	44·034
	For 30 days, the sun in Pisces	45·100
Evaporation		2·65 in.
Rain		1·60

Laboratory, Stratford, Fourth Month, 20, 1822. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1822.

Article I.

Account of a Volcanic Eruption in Iceland. By Dr. Forchhammer.

(To the Editor of the *Annals of Philosophy*.)

SIR, *April* 18, 1822.

The very low state of the barometer throughout a great part of Europe in the months of December and January, although not immediately followed by any eruption of the volcanoes in Italy, excited apprehensions of violent volcanic phenomena in Iceland; and in the month of March, letters were received in Copenhagen from which the following account is drawn up.

In the beginning of the month of September, the frost began on the east coast, and on the east part of the north coast of Iceland, with a violence that was quite unexpected after the experience of the preceding years. An amazing quantity of snow fell, and the Greenland ice surrounded the whole east and north coast accompanied as usual by continual snow and frost. It was remarkable that the fine weather continued on the south coast of the island till the beginning of November, the lowest state of the thermometer at Næss, near Reikiavig, being on the 23d and 24th of September = 41° Fahr. On the 19th of Oct. it suddenly fell to 23° Fahr. which lasted, however, only for one day, and before and after that time the temperature of the atmosphere was constantly above the freezing point, until on the 1st of November, when constant frost began.

The island, though frequently alarmed by earthquakes, had experienced no volcanic eruption since that famous one of 1783 and 1784 from the Skaptaa-Jokkul, which destroyed such a great part of the cultivated lands, except some small eruptions which were said to have taken place in the interior, far from the inhabited part of the island, and which passed away without attracting further notice, when in December, 1821, a new crater was suddenly formed on the Eyafjeld-Jokkul, a mountain of which, among the numerous volcanic eruptions, only a single one is mentioned, in the year 1612, when a great part of the ice of the mountain burst, and was thrown into the sea.

The Eyafjeld-Jokkul (known among sailors under the name of Cape Hekla) is the highest of all the mountains in Iceland; and, according to the last measurements, is 5666 feet high. It is the southernmost of the chain of mountains in which the dreadful eruptions in the middle of the last century took place, and at about equal distances from the Kolla and Hekla. From 1024 to 1766, 24 eruptions are recorded to have occurred. That part of the mountain where the crater was formed borders two sides the cultivated land, which belongs to the hundred (Syssel) of Rangarvalla, in the south part of the island.

The following account is an extract of a letter from M. Bryniulo Sivertsen, Minister at Holt, in Eiafields-boigden, to the Bishop of Iceland, M. Vidalin:—"The real crater is about five miles from my house at Holt. The fire made its way suddenly by throwing off the thick mass of ice which scarcely ever melts, and of which, one mass, 18 feet high, and 20 fathoms in circumference, fell towards the north, and, therefore, fortunately not over the village. At the same time, a number of stones of different sizes slipped down the mountain accompanied by a noise like thunder; no real earthquake, however, was felt. After this a prodigiously high column of flame rose from the crater which illumined the whole country round so completely, that the people in the house at Holt could see as perfectly at night as in the day time. At the same time much ashes, stones, gravel, and large half-melted pieces of the rock, were thrown about, some of which amounted to the weight of 50 pounds. In the following days, and until the new year commenced, a great quantity of fine powder of pumice fell in the surrounding country according to the direction of the wind, so that a thick bed of it covered the fields. It resembled the falling of snow, and penetrated through all openings into the houses, where it exhaled an unpleasant smell of sulphur. The eyes suffered extremely by this dust. At Christmas, a violent storm from the South, raged; it rained hard, which produced the good effect of blowing and washing away the ashes from the fields, so that they will do but little harm. We think ourselves extremely fortunate that so frightful a revolution in our immediate neighbourhood has produced no bad effects either on men or animals."

Another extract of a letter from M. Terve Johansen, the Provost at Breidebolstad, about 18½ miles to the west of the volcanoes, dated the 1st of February, 1822, gives the following additions:—“We still see the column of fire of the volcano shining with the same clearness as in the beginning, without, however, throwing lava into the inhabited part of the island. The ashes are greyish-white, have a sulphurous taste, and it is reported that they burn with flame when thrown into the fire. The ice of the Jokkul was twice broken, and an eye-witness has assured me that some of the pieces were three times as high as himself, and of many fathoms in circumference. Among the numerous half-melted stones, one has been found thrown to the distance of about five miles from the crater. We have had no accounts of the bad effects of this eruption either on men or animals. The thick mass of ashes spread over the land of Vester Eyafield and Oster Landoe, which began to occasion diseases among the sheep, has been blown away by a heavy storm, and since that time the wind has carried the ashes from the volcano into the uninhabited mountains; the diseases among the sheep soon disappeared.”

The third account is from M. Steingrim Johnson, Provost at Rangarvalla and Vestmamoesyssel, and written from Odde, about 30 to 35 miles to the W. of the volcano, dated Dec. 19, 1821.

“On Wednesday, Dec. 19, at twilight, and later in the evening, a reddish light appeared on the E. which was the more surprising, as it was clear.

Dec. 20.—At one o'clock in the afternoon, a number of rather shining clouds was seen collected about the top of the mountain above Eyafjeld-Jokkul, ESE from Odde; the clouds soon changed into a high column of smoke increasing in thickness and darkness. Though the weather was clear and calm, the smoke was carried to the south; at sunset, the eruption seemed to cease, but the smoke soon rose again, and even more violently than before. When it was dark, we clearly saw the moving and and sparkling flame; from which we concluded that the eruption must be violent. Afterwards we heard that it was on the east or south side of the Vesterjokkul, near Hudnasten, and opposite to the farmhouse of Skaale, in the parish of Holt.

Dec. 21.—There was a violent storm, and the fire was observed varying in intensity; clouds of smoke rose with great violence. They remained on the mountain, and to the west of the Jokkul, whose white brilliant colour was now destroyed by the shower of ashes.

Dec. 22.—The same phenomena; the clouds increased, and spread all over the sky, principally towards the south.

Dec. 23.—The same smoke. In Hvols-Reppen, and in this parish, the people believed that they saw the falling of ashes which came from the north-east. Afterwards we were told that a great quantity of them had fallen that night, and before, in the villages that were nearest to the volcano.

Dec. 24, 25.—The clouds of smoke remained on the same place, and in the same direction, as before; now and then the fire was observed on the place of the first eruption.

Dec. 26, 27.—Heavy storm from north-east; the clouds of smoke on the same place.

Dec. 28.—The weather began to get more calm; it seemed as if the column of clouds was divided into two, which took different directions by different currents of wind.

Dec. 29.—Weather calm and pleasant. The clouds of smoke moved towards the north and east over the ice mountains. Late in the evening a mild rain.

During this whole time, the cold was moderate, not exceeding 25° Fahrenheit, and sometimes it was 4° above the freezing point. It is reported that the water of the river which falls into the ————, and in the other rivers that come from the Jokkul and the surrounding mountains, had increased considerably during the first days of the eruption. In the vicinity of the volcano a constant rumbling noise was heard, now and then accompanied by a dreadful crash, as if the whole immense masses of stone and ice were going to fall together. The greater part of the ashes was fortunately carried towards the north, into uninhabited mountains and heaths, where also a great quantity of pumice has fallen."

In another letter from the same Provost, dated Feb. 23, it is said, "The clouds of smoke have not yet disappeared, and today they are increased. No ashes, however, have been observed during a long time, and the Jokkul has resumed its shining white colour, so that the rain and wind must have removed the ashes. The smoke greatly resembles the steam rising from boiling water, and certainly owes its origin to the fire below. Some imagine they have observed that the Jokkul has decreased, and is now lower near the crater, which certainly must now be larger than before, the column of clouds increasing in circumference. So it appears at least from this side from N. to S.; but whether the same has taken place in the other direction, from E. to W. I am not able to say. It has been reported that to the E. two other volcanoes have had eruptions, the mountains Katla and Oraefa Jokkul, but nothing is known about it. Since the eruption, the weather has become worse, owing to its unparalleled variableness, storms, and afterwards cold, and a great quantity of snow."

Dr. Thorsteinson, in a letter to Prof. Oersted, gives the following additions:—"Since the 1st of January, the violence of the eruption has been decreasing. Though the town of Reikiavig is about 74 miles from the volcano, the flame was observed there several times at night, when the weather was clear. People that recollect the eruptions of 1766 and 1783 think this trifling, but principally because it has done no harm. Though distant about 74 miles from the volcano, I thought that the weather became much milder after the eruption. Though the barometer was pretty low during the eruption, yet it was lowest on Feb. 8, when it was only 27·25 inches; but the fire did not increase, nor did we feel any thing like an earthquake; but near the volcano, they had constantly small shocks."

The vessel which brought this news had left Iceland on the 7th of March, and it is reported that the sailors when at sea again saw a violent fire.

* * *

The State of the Barometer and Thermometer from the Beginning of December to the End of February, at Næs, near Reikiavig, in Iceland. By Dr. Thorsteinson. (Reduced to English Measures and Fahrenheit's Thermometer.)

1821.	Barom.	Ther.	1822.	Barom.	Ther.	1822.	Barom	Ther.
Dec. 1	28·99	23½	Jan. 1	29·75	39	Feb. 1	29·34	12
2	28·64	23½	2	29·84	27½	2	29·35	14
3	28·75	12	3	29·90	27½	3	29·23	17
4	29·32	3	4	30·15	23½	4	29·07	23½
5	29·38	23½	5	30·18	34	5	29·05	17
6	29·43	27	6	30·18	34	6	27·99	17
7	29·46	20½	7	30·12	33	7	27·88	18½
8	29·49	18½	8	30·08	33	8	27·25	23½
9	29·55	18½	9	29·32	25	9	28·70	34
10	29·61	27	10	29·62	25	10	29·05	27
11	29·03	34	11	29·68	25	11	29·42	27
12	29·18	36	12	29·63	23½	12	29·32	21
13	29·12	39	13	29·49	25	13	29·16	34
14	29·18	27	14	29·48	23½	14	29 05	27
15	29·25	32	15	29·25	32	15	28·99	27
16	29·10	39	16	29·23	23½	16	29·57	25
17	29·12	41	17	29·60	25	17	28·56	27
18	29·16	41	18	29·52	23½	18	27·72	27
19	29·14	34	19	29·05	25	19	28·25	27
20	29·04	30	20	29·15	25	20	28·33	36
21	28·70	25	21	29·34	20	21	28·49	25
22	28·53	23½	22	29 78	23½	22	28·63	23½
23	28·57	25	23	29·84	25	23	28·45	18½
24	28·54	25	24	30·05	32	24	29·66	8
25	28·52	27	25	30·06	23½	25	29·68	9
26	28·49	30	26	30·02	30	26	29·60	11½
27	28·58	27	27	30·00	27	27	28·76	36
28	28·99	54	28	29·40	30	28	29·11	25
29	29·12	34	29	29·13	25			
30	29·13	36	30	28·96	25			
31	29·83	36	31	29·06	21			

Article II.

Astronomical Observations, 1822.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

April 30.	Occultation of *d* Leonis by the moon	Immersion	15h	08′	43·8″	Siderial time.
		Emersion	16	06	00·3	
May 1.	Occultation of υ Leonis by the moon	Emersion	10	30	36 0	Siderial time

Article III.

On a Clock with a Wooden Pendulum. By Col. Beaufoy, FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Bushey Heath, Stanmore, May* 16, 1822.

In the *Annals of Philosophy* for March, 1820, I was favoured by the publication of a paper describing a clock with a wooden pendulum, and its rate of going for 12 months; and in the same month of the following year, a similar table was inserted of the daily rate for a like period. I have now the pleasure of sending you a third year's register of the clock's diurnal variation. The rod has hitherto remained in its natural state, but I purpose trying the effect of covering it with varnish. If olive oil be exposed to the rays of the sun for a considerable length of time, it becomes colourless, limpid, free from mucilage, and not easily congealable. I have exposed two eight ounce phials, nearly filled with this oil, to the solar beams for one and two years, and consequently can speak to the fact. The bottle should be opened occasionally to allow the gas to escape, or the cork may be expelled. Chronometer makers would find this mode useful o treating the oil they commonly use for clocks and watches.

I remain, dear Sir, truly yours,

Mark Beaufoy.

Table containing the Rate of a Clock with a Wooden Pendulum.

Date.	Clock fast or slow.	Difference.	Daily rate.	Date.	Clock fast or slow.	Difference.	Daily rate.	Date.	Clock fast or slow.	Difference.	Daily rate.
1821.				1821.				1822.			
April 25	−0′ 02·18″	2·18″		Sept. 22	−1′ 43·89″	13·23″	−1·89″	Jan. 15	−2′ 00·78″	1·83″	−1·83″
26	−0 03·85	1·67	−1·67″	24	−1 46·67	2·78	−1·39	18	−0 04·48	3·70	−1·27
28	−0 05·29	1·44	−0·72	Oct. 1	−1 55·55	8·88	−1·27	21	−0 04·63	0·15	−0·05
May 2	−0 07·84	2·55	−0·64	2	−1 55·93	0·43	−0·43	25	−0 13·50	8·87	−2·22
5	−0 11·84	4·00	−1·33	5	−1 59·23	3·25	−1·08	27	−0 15·61	2·11	−1·06
14	−0 18·72	6·88	−0·76	†7	−2 00·80	1·57	−0·78	Feb. 6	−0 24·37	8·76	−0·88
16	−0 20·66	1·94	−0·97	8	−0 02·65	1·85	−1·85	10	−0 28·27	3·90	−0·98
19	−0 20·66	0·00	−0·00	10	−0 03·95	1·30	−0·65	13	−0 30·76	2·49	−0·83
24	−0 21·85	1·19	−0·24	13	−0 05·24	1·29	−0·43	15	−0 33·28	2·52	−1·26
28	−0 16·16	5·69	+1·42	16	−0 09·00	3·76	−1·25	17	−0 36·35	3·07	−1·53
31	−0 15·23	0·93	+0·31	18	−0 10·07	1·07	−0·53	19	−0 38·96	2·61	−1·30
June 1	−0 13·79	1·44	+1·44	21	−0 12·29	2·22	−0·74	21	−0 40·34	1·38	−0·69
5	−0 16·63	2·84	−0·71	25	−0 14·59	2·30	−0·58	23	−0 41·04	0·70	−0·35
14	−0 19·00	2·37	−0·27	29	−0 18·40	3·81	−0·95	27	−0 41·67	0·63	−0·16
18	−0 22·81	3·81	−0·95	Nov. 4	−0 29·52	11·12	−2·22	March 1	−0 41·39	0·28	+0·14
26	−0 27·71	4·90	−0·61	5	−0 29·87	0·35	−0·35	3	−0 40·86	0·53	+0·26
29	−0 27·84	0·13	−0·04	6	−0 29·96	0·09	−0·09	12	−0 42·37	1·51	−0·17
July 5	−0 33·38	5·54	−0·92	8	−0 31·10	1·14	−0·57	15	−0 42·61	0·24	−0·08
9	−0 35·26	1·88	−0·47	12	−0 34·85	3·75	−0·94	21	−0 46·87	4·26	−0·71
12	−0 37·77	2·51	−0·84	17	−0 44·50	9·65	−1·93	23	−0 47·88	1·01	−0·50
16	−0 38·97	1·20	−0·30	21	−0 51·07	6·57	−1·64	28	−0 46·89	0·99	+0·20
18	−0 43·55	4·58	−2·29	25	−0 53·43	2·36	−0·59	April 3	−0 41·70	5·19	+0·86
21	−0 46·47	2·92	−0·97	28	−0 55·24	1·81	−0·60	6	−0 38·95	2·75	+0·92
26	−0 52·05	5·58	−1·12	30	−0 59·29	3·96	−1·98	8	−0 36·33	2·62	+1·31
*Aug. 2	−0 61·96	9·91	−1·41	Dec. 2	−0 59·80	0·06	−0·30	11	−0 30·76	4·43	+1·48
4	−0 06·42	4·46	−2·23	6	−1 05·01	5·21	−1·30	18	−0 27·25	3·51	+0·50
10	−0 15·81	9·39	−1·56	11	−1 12·20	7·19	−1·44	19	−0 26·60	0·65	+0·65
13	−0 16·19	0·38	−0·13	19	−1 26·59	14·39	−1·79	21	−0 24·72	1·88	+0·94
17	−0 26·38	10·19	−2·55	23	−1 30·80	4·21	−1·05	23	−0 23·25	1·47	+0·74
19	−0 31·65	5·27	−2·63	25	−1 32·57	1·77	−0·88	29	−0 19·05	4·20	+0·70
21	−0 36·79	5·14	−2·57	31	−1 37·50	4·93	−0·82	May 3	−0 14·52	4·53	+1·13
24	−0 39·57	2·78	−0·93	1822.				6	−0 12·40	2·12	+0·71
25	−0 40·97	1·40	−1·40	Jan. 2	−1 40·28	2·78	−1·39	11	−0 08·67	3·73	+0·32
Sept. 1	−0 52·62	11·65	−1·66	5	−1 43·45	3·17	−1·06	14	−0 06·86	1·81	+0·60
12	−1 22·70	30·08	−2·74	8	−1 47·10	3·65	−1·22				
15	−1 30·66	7·96	−2·65	14	−1 58·95	11·85	−1·97				

Previous to the commencement of these observations, the length of the pendulum was altered.

* Clock put forward one minute.

† Clock put forward two minutes.

Article IV.

On the Motions produced by the Difference in the Specific Gravity of Bodies. By Mr. Charles Sylvester.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR, *Great Russel-street, April 22, 1822.*

All the writers on dynamics have treated largely on the effects of fluids in motion, as regards their resistance and their direct impulse on surfaces, without entering scarcely at all into the causes of their motion, and particularly those motions resulting from, or affected by, a difference of specific gravity. Prof. Robinson, and several other authors, have given us the principles and excellent formula for calculating the different results of air rushing into a vacuum and into any other rarified medium approaching a vacuum; but these do not at all apply to the subject I here wish to treat. The ordinary formula and calculations for falling bodies are all made on the supposition that the effects take place in vacuo. They, of course, will not apply when the body falls in a medium of the least possible density. The data relative to falling bodies are:

h = the height the body falls from.
t = the time of falling.
v = the velocity acquired by the fall.
g = the space a body falls through in one second = $16\frac{1}{12}$.

Then since a body in falling through any space acquires a velocity which would take it through twice that space in the same time; the velocity of g will be equal to $2\,g$.

The body would, therefore, pass through $2\,g$ in the next second, if gravity were to cease, but it causes the body to pass through another g in the second second, making the whole $3\,g$; this with the one g in the first second makes $4\,g$. It will be found by similar reasoning that it will have passed over $9\,g$ at the end of the third second, and so on as the square of the time. The velocity acquired in each successive second will be $2\,g$ for the first second, $4\,g$ for the second, $6\,g$ for the third, and so on. Hence it will appear that the whole space passed over in any time is as the square of that time, and as the square of the velocity; and that the velocity is, therefore, as the time. It may appear unnecessary to a mathematician to give these particulars, presuming that those likely to read this article will be acquainted with such elementary knowledge. But I do it with the hope that some may read it, who would have stopped short of what I have in future to communicate from the want of such elementary knowledge.

From the above reasoning, we shall have as the square of one second is to g, so is the square of t or t^2 to h; that is, by multiplying means and extremes, we have $h = g\,t^2$. By taking the same course with the velocity, which for one second (from what has been observed above) will be $2\,g$, we have as the square of $2\,g$ or $4\,g^2$ is to g, so is v^2 to h.

This gives $4\,g^2,\ h = g\,v^2$, and $v^2 = 4\,g\,h$.

From the first expression, $t^2 = \frac{h}{g}$ and $t = \sqrt{\frac{h}{g}} = \frac{h^{\frac{1}{2}}}{g^{\frac{1}{2}}}$; from the second expression $v = 2\,g^{\frac{1}{2}}\,h^{\frac{1}{2}}$, and $h = \frac{v^2}{4\,g}$. These formulæ, as has been observed, are true only when bodies fall in vacuo. In the atmosphere, or any other medium, the velocity acquired by falling through a given space will be less than the above in the inverse ratio of the difference between the specific gravity of the body and the medium in which it falls. The difference of specific gravity in this case is precisely the same in effect with the difference of the absolute gravity of two unequal weights at each end of a cord, and hanging over a pulley. If the weights are equal, it is plain that no motion will take place, since the only moving force is the difference of the weights, and the matter to be moved equal to the sum of these weights. Hence if h (as above) be the height which the heavy weight would fall through, we shall have the following formula for the velocity, viz. $v = 2\,g^{\frac{1}{2}}\,h^{\frac{1}{2}} \times \frac{W - w}{W + w}$, W and w being the weights. It will be evident that when $W = w$, v equals nothing, and when w equals nothing, v will be the same as the first formula would give. The motion of bodies in different media is affected in the same way by the difference of specific gravity, and the theorem is the same, with the exception of the denominator being the greater specific gravity, and not the sum of the two as with the motion of the unequal weights.

Let $S =$ the specific gravity of a body greater than the medium in which it falls, and $C =$ the specific gravity of the medium; then we shall have, agreeably to the above theorem, $v = 2\,g^{\frac{1}{2}}\,h^{\frac{1}{2}} \times \frac{S - C}{S}$. To make this more familiar, let $S = 9$, being the specific gravity of copper nearly, and $c = 1$, being the specific gravity of water; and let $h =$ four feet. Then $v = 2 \times \sqrt{16} \times \sqrt{4} \times \frac{9 - 1}{9} = 14\frac{7}{9}$, being $1\frac{7}{9}$ feet less than if the body had fallen through the same space in vacuo which would be 16 feet.

When the difference of specific gravity is very small, the effect of the medium is more conspicuous, as is well known in the experiment of the feather and the guinea. When bodies fall in vacuo, the velocity is accelerated during the whole time of falling, whatever may be the space to fall through; but in falling through a medium of any density, the velocity has a maximum at which it arrives when the resistance of the medium is equal to the force of gravity. After this, the body moves with a uniform velocity. The time in which the body attains this maximum is as the ratio between the surface and weight of the body, which may be a ratio of any assignable magnitude, limited only by the extent to which matter is divisible. This will be obvious when it is known that in so dividing matter, the weight decreases as the cube of the diameter; while the surface diminishes only as the square of the same. Hence matter of the greatest specific gravity may be so divided that it may acquire a maximum velocity by falling through the least possible space, acquiring a uniform velocity of any assignable value. A body, whatever may be its figure, by falling in a fluid, will describe a prism, the base of which will be the surface which moves against the fluid. When the prismatic column of the fluid, formed by the falling body, becomes equal to the weight of the body, the resistance of the fluid will be equal to the force of gravity, and consequently the body will, from that point, move with a uniform velocity. If the surface presented to the fluid during its motion be a hemisphere, it will meet with only half the resistance which would be given to a plane surface.

Let a equal the surface presented to the fluid.
h = the height it will fall from to acquire a uniform velocity.
f = the specific gravity of the fluid.
B = the specific gravity of the body.
l the length of the solid prism.

Then $f : \mathrm{B} :: l : h \therefore l = \frac{f\,h}{\mathrm{B}}$, and this is the same whatever may be the value of a.

When B is less than f, then the body will rise in the fluid, and will attain its greatest velocity, when by its rising, it has formed a prism equal in weight to its own bulk of the fluid, having its own area of base and specific gravity. In this case, $l\,\mathrm{B} = f\,h$, and l is greater than h in the ratio that f is greater than B.

What will be the length of a prism of lead in order that it may acquire its greatest velocity by falling through one foot of water?

In the theorem $l = \frac{f\,h}{\mathrm{B}}$, $h = 12$ inches, $\mathrm{B} = 11$, and $f = 1 \therefore$ $l = \frac{1 \times 12}{11} = 1\frac{1}{4}$ inches, the length of the prism. If $f = \frac{1}{840}$ being the specific gravity of air, the value of l will be obtained for a prism falling in the atmosphere, in which case $l = \frac{1}{770}$ of an inch whatever may be the area of the surface. If the falling body be a sphere, then it will meet with only half the resistance of a cylinder or prism with its flat face perpendicular to the direction of motion.

Now let $x =$ the diameter of the sphere, and also of the prism of the fluid, which is to be equal to the weight of the sphere.

$h =$ the height of the last prism.
$v =$ the velocity acquired by a body falling through h.
$B =$ the specific gravity of the body.
$f =$ that of the fluid.
$c = 3{,}1416$; and
$g = 16\frac{1}{12} =$ the space a body falls through in one second.

Then the content of the sphere will be $\frac{c\,x^3}{6}$, and that of a prism of the fluid of the height the body falls from, to acquire its greatest velocity, will be $\frac{c\,x^2\,h}{4}$, which would be equal to the resistance, if the surface were A flat, and perpendicular to the direction of the fall, but the sphere has only half that resistance.

Hence $\frac{(B-f)\,c\,x^3}{6} = \frac{c\,x^2\,h\,f}{4 \times 2}$, and $x = \frac{3\,h\,f}{4\,B-f} = \frac{3\,v^2\,f}{16\,g\,B\,f}$.

What will be the diameter of a sphere of lead to fall at the rate of one foot per second after it has attained its maximum. In this case, $v = 1$, and $h = \frac{1}{64} = \frac{12}{64}$ inches $= \frac{3}{16} = \cdot1875$.
$B = 11$, specific gravity of lead.
$f = \cdot00119$, that of air (water being 1).

$B - f = \cdot99881$. $x = \frac{3 \times \cdot1875 \times \cdot00119}{4 \times 10\cdot99881} =$ 0000152 inches nearly. The maximum velocity of the same sphere in water will be obtained by putting, instead of h, $\frac{v^2}{4}$. Hence we get $3\,v^2\,f = 16\,g\,x\,(B-f)$ and $v = \left(\frac{16\,g\,x\,(B-f)}{3\,f}\right)^{\frac{1}{2}} =$

$$\frac{16 \times 16 \times \cdot000001266 \times (11-1)}{3 \times 1}\Big)^{\frac{1}{2}} = 0329$$ feet per second, and 1·974 feet = per minute.

If the sphere were of a specific gravity of 2, about that of earthy matter, by which water is sometimes rendered muddy, its uniform velocity would be ·6414 feet per minute. The falling of various precipitates in chemical experiments must depend upon their specific gravity, and the size of the particles; data might be obtained from an experiment by observing the time in which bodies are falling, the diameters and specific gravities being known, in order to find the magnitude of particles of other matter, the time of falling and their specific gravities being known.

Very small shot of a given size might be made to fall in a long glass tube filled with water, and the time of falling observed; from this data, if the time of any other matter falling be given, the height fallen from and specific gravities being known, the size of the particles may be found. The unequal times for the subsidence of different precipitates shows a striking difference either in the specific gravities or the size of the particles. The earths and their insoluble salts are longer subsiding than those of the metals, and the latter subside quicker as their specific gravities are greater. We hence are enabled to explain how large quantities of matter may exist in the atmosphere and in other fluids apparently in a state of suspension, but in reality falling with a velocity inversely proportionate to their size and specific gravities. This in the atmosphere is visible to the eye, when viewed in a sun-beam, and is principally animal and vegetable matter. This explains the cause of the air becoming contaminated when heated to a temperature approaching to a red heat at which the foreign matter is decomposed.

We are also enabled to account for the aqueous matter in the atmosphere in the form of clouds; this has formerly been supposed in a state of suspension in some intermediate form between water and vapour: this as well as the visible matter observed when aqueous vapour is condensed, is nothing more than globules of real water, there being no intermediate state between real water and its elastic invisible vapour. Hence we have frequently in the atmosphere spherical masses of real water from the size of the largest drops of rain to those which are so minute as not to reflect light sufficient to make them visible.

Supposing steam or water in the elastic form (which can exist at all temperatures) to consist of distinct atoms, each surrounded by an atmosphere of caloric, the smallest particle which could exist in the state of water, would consist of two individual atoms, the next three atoms, and so on till the particle becomes visible.

The first appearance of condensation is a slight opacity, which soon becomes whiter, as is observed in mist or fog. As the particles become larger, they fall with more rapidity, and assume a darker colour, as is observed in various states of clouds. By having recourse to the formula above given, we may form some idea of the size of the globules of water to give an appearance of their being suspended, in which state clouds have been supposed to exist. In this case, as before, we put x = the diameter of a particle, or globule of water.

What must be the diameter of a globule of water to fall at the rate of one inch per second after it acquires a uniform velocity?

Here $v = 1$ inch.

$f = \frac{1}{840} = \cdot 00119.$

$g = 16\frac{1}{12}.$

$B = 1.$

$B - f = \cdot 9988.$

$$x = \frac{3 v^2 f}{16 g (B - f)} = \frac{3 \times 1^2 \times \cdot 00119}{16 \times 16 \times 12 \times \cdot 9988} = \cdot 000001164 \text{ inches.}$$

When the body is of less specific gravity than the liquid, it rises as a cork rises in water; although in these cases a principle of levity seems to operate, the effect is to be attributed to the superior specific gravity of the fluid, and is relatively the same as if a body of the same specific as the fluid were to fall through a fluid of the same specific gravity as the body, the difference of specific gravity being the moving force. In order to ascertain the velocity with which a certain volume of cork would rise in water, the same result would be obtained by considering the fluid as being of the specific gravity of the cork, and a solid of the volume of the cork, but of the specific gravity of the fluid, the velocity at any point will be the same, the direction being upwards instead of downwards.

What will be the greatest velocity which a sphere of cork of one inch in diameter can acquire in rising through a column of water? The theorem $v = \left(\frac{16\,g\,x\,(\,.\,B + f)}{3\,f}\right)^{\frac{1}{2}}$ will become $v = \left(\frac{16\,g\,x\,(f - B)}{3\,B}\right)^{\frac{1}{2}}$. Let $x = 1$ inch $= \cdot0833$ feet; B$=$and $\cdot24\,f = 1$, then $v = \frac{16 \times 16 \times \cdot083 \times \cdot76}{3 \times \cdot24} = 22\cdot5$ feet nearly.

What must be the diameter of a sphere of cork to acquire a uniform velocity of one inch per second?

Here $x = \frac{3\,v^2\,B}{16\,g\,(f - B)}$; this will give x in feet; and $x = \frac{3 \times 1^2 \times \cdot24}{12 \times 16 \times 16 \times \cdot76} = \cdot000312$ in inches.

When fluids of different specific gravities are mixed together having no chemical affinity for each other, as is the case with oil and water, or water and mercury, the lighter fluid ascends with the same velocity, and in every respect is similar to a solid body of the same specific gravity with the fluid, if the lighter fluid be kept in a distinct volume, as is the case with a balloon. It is exactly the same as if a body of uniform density having the mean specific gravity of the gas, and the matter containing it, and the theorem applied to the case of cork rising in water, would equally apply in this case. If a constant stream of the lighter fluid be introduced into the denser one, and the succession kept up, a uniform velocity will be established which will be as the square root of the height, and as the difference of density between the two fluids to the greater density. It will be the same thing whether the fluids are naturally of different specific gravities, or the volume of one part of the fluid be changed by a change of temperature. This may be illustrated by filling a tall glass vessel with hot water containing small pieces of amber, a substance very nearly of the same specific gravity with water; the sides of the vessel exposed to the air become cooled, and the water in contact loosing its heat becomes specifically heavier and descends; and as this is taking place all round the vessel, an upward current takes place in the middle of the column caused by the descending current, as may be observed by the direction of the particles of amber. If, instead of putting hot water into the vessel, cold water were employed, and the bottom of the vessel heated, the heated part becoming specifically lighter, immediately gives place to the heavier fluid which is colder, and the same appearance will take place as before; the currents remaining permanently uniform, so long as there exists the same difference of temperature between the different parts of the fluid.

This is what takes place in the atmosphere when partial heat is applied; the heated column in proportion to the rarefaction is pushed upwards by the surrounding dense column. We have no instance of this kind so familiar as the ascension of the smoke and heated air in chimneys. It has been always found that the velocity of the ascending current was greater as the chimney was higher, and in proportion to the heat applied at the bottom.

Many years ago having occasion to write an article on furnaces for a popular work, I found that the power of draft in chimneys was not strictly as the height, but as the square root of the same; and as the ratio of the difference of density between the rarified column and the outer air to the density of the outer air, I also found that under the greatest rarefaction that could be given, the velocity was always less than a heavy body would acquire by falling through the height of the chimney.

If V be the velocity which a body would acquire by falling through the height of the chimney, D, the density of the outer air, d that of the air in the chimney, and v the velocity of the heated current; then $v = \mathrm{V} \times \frac{\mathrm{D} - d}{d}$, as has been shown with bodies rising or falling in a fluid. If h = the height of the chimney, or any other column of heated air, then $v = 2\, g^{\frac{1}{2}}\, h^{\frac{1}{2}} \times \frac{\mathrm{D} - d}{d}$.

An essay on this subject has lately appeared in a respectable periodical work in which the principle of air rushing into a vacuum is assumed to calculate the relative changes of heated air as applied to the ventilation of buildings. We do not require any stronger proofs of the incorrectness of the principle on which the calculations are founded than the results given, in which the velocity of a current is made about five times greater than a body would acquire by falling through the height of the chimney. The formulæ above given agree very nearly with practice, and may be made more useful by getting the value of d in terms of the temperature, D being always that density which the temperature of the atmosphere would give. For this purpose, let T = the temperature of the atmosphere by Fahrenheit's scale, t that of the heated column, and let e = the expansion or the increase of a volume of air by one degree. Then $t - \mathrm{T}$ will express the difference of temperature between the atmosphere and the heated column. If, therefore, the volume at T be called 1, that at one degree above will be $1 + e$, and that at $t - \mathrm{T}$ will be $(1 + e)^{t - \mathrm{T}}$.

Then since the densities will be inversely as the volumes, we have as $\mathrm{D} : d :: (1 + e)^{t - \mathrm{T}} : 1$.

Hence $\mathrm{D} - d = 1 - \frac{1}{(1 + e)} t - \mathrm{T}$ and $v = 2 g^{\frac{1}{2}} h^{\frac{1}{2}} \left(1 - \frac{1}{(1 + e)} t - \mathrm{T}.\right)$ The temperature of a chimney being 120°, that of the outer air 40°, and the height of the chimney 40 feet, what will be the velocity of the current?

In this case, $v = 2 \times 4 \times \sqrt{40} \times 1 - \frac{1}{(1 \cdot 00208)^{80}} = 7 \cdot 7364$.

In getting the denominator of the fraction, it may be convenient to have the logarithm of 1·00208, which is ·0009038.

I am, yours truly,

C. Sylvester.

Article V.

On the Temperature of Mines in Cornwall. By Mr. M. P. Moyle.

(To the Editor of the *Annals of Philosophy*.)

Sir, *Helston, May* 3, 1822.

When I asserted in the *Annals of Philosophy* for April last that Mr. Fox had drawn false conclusions as regards the temperature of mines in this county, the impression on my mind was, that sufficient attention had not been paid to the selection of spots free from doubtful causes, but he assures us in your last number that every precaution was made to obviate such effects. I hope this was the case; as I am fully convinced from the experiments which I have made, that it is otherwise impossible to get near the truth; and I most sincerely hope that Mr. Fox does not feel offended at any remarks of mine which may be in opposition to his own, as I conceive it is only by the collision of facts by different individuals that the truth can be elucidated.

I would now remark that unless we can select spots where workmen have no access, and where the air and water are in a state of rest, it will be impossible to gain the truth. A favourable opportunity of this description occurred to me five days since in Wheal Trenoweth Mine, 100 fathoms east of Crenver and Oatfield Mines on the same load. This mine has discontinued working (I believe) for more than 12 months, or at least as far as regards the presence of miners. The adventurers of the above united Mines still keep the engine (a water wheel) of Wheal Trenoweth working for the purpose of relieving the burden of their own engines.

The adit at which the water is discharged is 32 fathoms from the surface: here the water from Wheal Trenoweth 100 fathoms from where it was drawn up was 54°. This water gradually increased in temperature from this place, where we descended, to the mouth of the pump, where the water drawn from the bottom was 56°: 15 fathoms deeper the walls of the shaft were 54°; a gallery at this level 40 fathoms east of the shaft was only 53°; and five fathoms deeper still, or 52 from the surface where there is a second cistern of water, the water was 57°; the walls at the same time 54½°, and at the bottom 66 fathoms, or 396 feet, the water that ran through a small crevice, as well as the walls of the shaft, were still 54°. The temperature at the surface before descent was 62°; on our return 64°.

Here I would say that there is positive proof of no increase of temperature for 34 fathoms, being precisely the same at the bottom as at the adit level.

The increase of heat in the water at the cistern, and at the mouth of the pump, I can only attribute to the friction of the machinery, which certainly appeared to be very great at the time.

To show the influence of a few persons on the temperature of the air of a small mine, I found on our return (being three persons) that the air was 1° warmer than at our descent at the adit level.

Possibly I might be mistaken should I assert that the internal strata of the earth *generally* are not warmer than the mean of the surface. I do not find that 53° is at all too high for this mean, and should we be able to find a single instance at the bottom of some of our deep mines where the temperature is not above the mean, I conceive Mr. Fox's theory must be relinquished.

Some of my experiments were made several years since, and, perhaps, not with that degree of accuracy with which they ought to have been. I have, therefore, refrained from giving them at present. The above may be relied on. I mean to prosecute this subject further; and that by various means, particularly by sinking a self-registering thermometer to the bottom of some of our oldest and deepest mines long ceased working, and consequently full of water. I think this the only certain means of arriving at the wished-for accuracy. The facts when collected I shall certainly present to the public.

I am, Sir, your humble servant,

M. P. MOYLE.

ARTICLE VI.

Some Observations on Neutral Series. By W. Marrat, AM. Member of the Philosophical Society, New York, and private Teacher of Mathematics, Liverpool.

(To the Editor of the *Annals of Philosophy.*)

SIR,

THE following neutral series $1 - 1 + 1 - 1 +$ &c. to infinity has exercised the talents of some of the greatest mathematicians since the time of Leibnitz, who was among the first that noticed it. Dividing 1 by $1 + a$, we obtain the following series, viz. $1 - a + a^2 - a^3 + a^4 -$ &c.; and by taking the value of a equal to 1, it produces the series of units above given, because every power of unity is 1. Leibnitz makes the above series, when $a = 1$, equal to $\frac{1}{2}$, and in that one particular case it is true. Euler notices the same series in his algebra. "This series," he observes, "appears rather contradictory; for if we stop at $- 1$, the series gives nothing, and if we stop at $+ 1$, it gives 1. But this is precisely what solves the difficulty; for since we must go on to infinity without stopping either at $+ 1$ or $- 1$, it is evident that the sum can neither be 0 nor 1, but that the results must lie between the two, and, therefore, be equal to $\frac{1}{2}$." In the case before us, where the series is deduced from the expression $\frac{1}{1 + a}$, it becomes, when $a = 1$, $\frac{1}{1 + 1} = 1 - 1 + 1 - 1 +$ &c. ..... and the series is evidently equal to $\frac{1}{2}$. The result, as Euler observes, certainly lies between 0 and 1, but it is not universally equal to $\frac{1}{2}$; for $\frac{1}{1+1+1} = \frac{1}{3} = 1 - 1 + 1 - 1 +$ &c..... gives also the same series as that above for $\frac{1}{2}$; and again $\frac{1}{1+1+1+1} = \frac{1}{4} = 1 - 1 + 1 - 1 +$ &c.... which is still the same; and the series will be the same whatever may be the number of *ones* in the denominator of the fraction from which it is produced. The arithmetical mean then between 1 and 0 only gives the value of the series in one particular case out of an infinite number. Again, we have $\frac{1+1}{1+1+1} = \frac{2}{3} = 1 - 1 + 1 - 1 +$ &c..... or $\frac{1+1+1}{1+1+1+1} = \frac{3}{4} = 1 - 1 + 1 - 1 +$ &c. .... and the same series will always be produced if the number of *ones* in the denominator exceed the number in the numerator; that is, the series will be produced, if nine units be divided by ten units, or 99 units by 100, or 999 by 1000; whence it is obvious that the series is equal to any fraction less than unity, which is its greatest limit; the other limit is zero, as we shall soon demonstrate.

Let us reduce into series,

$$\frac{1}{1+x} = 1 - x + x^2 - x^3 + x^4 - x^5 + \&c. \ldots \text{ to infinity.}$$

$$\frac{1}{1+x+x^2} = 1 - x + x^3 - x^4 + x^6 - x^7 + x^9 - \&c. \ldots$$

$$\frac{1}{1+x+x^2+x^3} = 1 - x + x^4 - x^5 + x^8 - x^9 + x^{12} - \&c. \ldots$$

$$\frac{1}{1+x+x^2+x^3+x^4} = 1 - x + x^5 - x^6 + x^{10} - x^{11} + x^{15} - \&c. \ldots$$

The first of these series contains the successive powers of a, or it may be said to contain all the terms; and when $x = 1$, it becomes $\frac{1}{1+1} = \frac{1}{2} = 1 - 1 + 1 - 1 +$ &c. to infinity.

The second series wants several of its terms, and on that account it ought to be less than the first; however, when $x = 1$, it becomes $\frac{1}{3} = 1 - 1 + 1 - 1 +$ &c. .... to infinity.

In the third series, more of the terms are wanting, and this series should be less than the second series; however, when $x = 1$, it becomes $\frac{1}{4} = 1 - 1 + 1 - 1 +$, &c. .... to infinity.

In the fourth series, several more of the terms are gone; but when $x = 1$, we have $\frac{1}{5} = 1 - 1 + 1 - 1 +$ &c. .... to infinity.

It appears from all these expressions that the two first terms remain constant, but that the other terms gradually disappear, according as the number of terms in the denominator increase; whence it also appears from the law of continuity, that the value of the series ought regularly to diminish.

We will now determine the series, when the denominator consists of an infinite number of terms; thus let

$$\frac{1}{1+x+x^2+x^3+\&c. \ldots \text{ to infinity}} = A + Bx + Cx^2 + Dx^3 + \&c. \ldots$$

Multiplying the second side of this equation by the denominator of the first, we have

$$\begin{array}{rrrrl} A + & Bx + & Cx^2 + & Dx^3 + & \&c. \\ & + Ax + & Bx^2 + & Cx^3 + & \ldots \\ & & Ax^2 + & Bx^3 + & \ldots \\ & & & + Ax^3 + & \ldots \end{array}$$

and equating the coefficients of the same powers of x, $A = 1$, $B + A = 0$, $C + B + A = 0$, $D + C + B + A = 0$; that is, $A = 1$, $B = -1$ $C = 0$, $D = 0$, &c.; therefore,

$$\frac{1}{1+x^2+x^3+x^4+\&c. \text{ ad infin.}} = 1 - x,$$ where all the terms, except the two first, or $1 - x$, have vanished. Let $x = 1$, then

$$\frac{1}{1-1+1-1+\&c. \text{ to infinity}} = 1 - 1 = 0;$$ whence it is manifest that when the denominator consists of an infinite number of

units, the value of the series is 0; the value of the series then always lies between 0 and 1.

We observe then that whatever may be the number of units in the denominator, if that number be less than infinite, the form of the series is still the same. It would appear from the preceding investigation, however, that the value of the series diminishes continually from 1 to 0, and that the value of the series 1 — 1 + 1 — 1 + &c. to infinity varies from 1 to 0.

It is well known that a neutral series is the limit between a diverging and a converging series; may we not then from this circumstance, and what is shown above, conclude that such a series has no determinate value; at least that it has no value that can be determined from the series itself by a direct investigation. If the series 1 — 1 + 1 — 1 + &c. .... be said to have a determinate value, or limit, we may naturally ask, which of its values is meant? Or if I should chance to meet with the above series in any calculations, what vulgar fraction ought I to substitute as its value; for it has been proved above to be equal to $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c. to infinity; or to $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$, &c. to infinity, beside a great many others. We may conclude then that $\frac{1}{2}$, instead of being universally the value of the series 1 — 1 + 1 — 1 + &c. to infinity, is only *one particular value* out of an *infinite number.*

ARTICLE VII.

Remarks on Dr. Thomson's Paper "On the Influence of Humidity in modifying the Specific Gravity of Gases." By John Herapath, Esq.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cranford, London, April* 11, 1822.

DR. THOMSON having, for the expressed purpose of "calling the attention of chemists" to the subject, published in the *Annals* for April some views on vapour and latent heat, I am induced, as a branch of science to which I have paid a little attention, to offer some remarks in the Doctor's ideas.

"*Whatever be the temperature of steam from* 212° *upwards,*" says Dr. Thomson, "*if we take the same weight of it, and condense it by water, the temperature of the water will always be elevated the same number of degrees.*" "It follows from this general law that the latent and sensible heats of steam (reckoning from 32°) added together always form a constant quantity, whatever be the temperature of the steam." From these principles, the Doctor has calculated a table exhibiting the "latent and sensible heats" of steam to about every 50°, from 32° to

500°. This table generalized may be represented by the formula

$$\text{Latent heat} + F - 32 = 1196, \text{ or latent heat} = 1228 - F,$$

in which F is the Fahrenheit temperature of the steam. Therefore as the temperature of the steam increases, the latent heat of it diminishes, and at 1228° is nothing. Hence at 1228° water may change its state; that is, from the fluid to aeriform, or *vice versâ*, and neither "*combine with nor separate from caloric.*" Now in Dr. Thomson's Chemistry, vol. i. p. 73, he tells us that the law of Dr. Black, who was the contriver of this latent heat, "in its *most general* form, may be stated as follows: *Whenever a body changes its state, it either combines with caloric, or separates from caloric.*" Hence Dr. Black's law "in its *most general* form" is in direct opposition to a "general law" which Dr. Thomson has deduced from the experiments of Messrs. Clement and Sharpe. What other conclusion can be drawn from these jarring and contradictory laws but this, I really cannot perceive; namely, that either Dr. Black's law of latent heat "in its most general form" is wrong, or the experiments containing "the property of vapours," to which Dr. Thomson is anxious to draw the attention of chemical philosophers, are themselves incorrect.

I shall make no observation on the results given by the experiments of Ure and Rumford, which I have discussed in another place, and shown to be very contrary to those it seems flowing from Sharpe and Clement's; but there is a consequence of the latter experiments so truly strange, that I find a difficulty in conceiving how it can be even possible. When water is heated above 1228°, the latent heat by these experiments becomes negative; that is, instead of heat being lost by the conversion of water into steam, there is an acquisition of heat; and conversely, by the conversion of steam into water, there is a loss of heat. Consequently if when the temperature is above 1228° steam is to be generated, and the same temperature maintained, we must not, as in other cases, increase the heat, but diminish it, and lighten the pressure a little. And in order to condense the steam and preserve the same temperature, we must somewhat compress the steam, and increase the temperature! Can this be right? Can nature work by laws so utterly repugnant to all analogy and parallel, and so positively contradictory to themselves? I know not whether I can myself see correctly, but I cannot perceive any difference in the relation of steam to water at 1500°, and in the same relation at 1000°. The one appears to me to be as much an air, and the other as much a fluid at the former temperature as at the latter; and thence I should conclude the conversion of water into steam at either temperature is still the same operation on the same fluid; and must consequently require the same means or cause.

It has been found that water is more easily distilled; namely, with a less expense of fuel, at a high temperature than at a low; and that high pressure steam engines do more work with less fuel than low pressure ones. These things Dr. Thomson advances as proofs of his inference of the diminution of latent heat. Now Mr. Dalton has shown by experiment that water evaporates nearly as the superincumbent pressure; and, therefore, at 340°, the generation of steam is about nine times as great as it is at 212°, though the temperature is only about one-eleventh part higher. Therefore, if nothing else interfered by increasing the temperature from 212° only about one-eleventh, we might perform nine times the work. To explain the economy of high pressure engines, we want, therefore, no new doctrine of latent heat; and hence the introduction of it appears to me to be superfluous.

By the experiments of Mr. Sharpe and Mr. Southern, the specific gravity of steam in conjunction with its fluid has some proportion to its elasticity. In the last volume of the *Annals*, p. 269, I have shown that this specific gravity and elasticity cannot be correctly proportional under any consideration whatever, neither from the theory I have advanced, nor from the experimental results of that which is usually admitted. Dr. Thomson, however, in his present paper, having assumed that the proportion is correct, I shall concisely show that it cannot be. Messrs. Dalton, Gay-Lussac, Dulong, and Petit, have proved that the volumes being the same, the elasticities; and the elasticities being the same, the volumes of a given portion of any gas at the temperature of 32° and 212° are as 8 to 11; and that in other cases, the increments or decrements of the elasticity or volume are proportional to the ascending or descending Fahrenheit temperature. Therefore when the volume is the same, the

$$\text{elasticity} = 448 + F;$$

and when the elasticity is the same, the

$$\text{volume} = 448 + F,$$

F denoting the corresponding Fahrenheit temperature. It has also been found that at the same temperature the specific gravity is proportional to the elasticity. And it has likewise been established by the concurrent experiments of the French and English philosophers, that with the exception of their not being able to sustain more than a certain pressure according to the temperature, vapours are perfect gases, and follow precisely the same laws of expansion and contraction. Let S be the specific gravity of vapour, and τ the tension at the temperature F, then at any other temperature F′, the elasticity of this same vapour under the same volume, would be

$$\tau \times \frac{F' + 448}{F + 448},$$

S being the specific gravity as before. If, therefore, τ' be the tension of vapour at the temperature F′, and S′ its specific gravity, we have

$$\tau \times \frac{F' + 448}{F + 448} : \tau' :: S : S' = S \times \frac{\tau'}{\tau} \times \frac{F + 448}{F' + 448}.$$

Hence it is impossible that the specific gravity of vapour can be accurately proportional to its tension; and this follows not from any new principles, but from what has been allowed by every philosopher. We may, therefore, safely affirm, that the table of the specific gravity of aqueous vapour Dr. Thomson has computed in his present paper for the rectification of the specific gravity of gases, cannot be correct.

If, as Gay-Lussac makes it, the specific gravity of vapour at 212° be $\frac{5}{8}$, that of common air at the same temperature, then the specific gravity of the vapour in any gas over water at the temperature F′ will be

$$\frac{5}{8} \times \frac{\tau'}{30} \times \frac{660}{F' + 448} = \frac{55\,\tau'}{4\,(F' + 448)},$$

supposing that the tension at 212°, and the elasticity at F′, are each 30, and the specific gravity of the atmosphere at 212° unity. Should the specific gravity of the vapour be required when that of our air is unity at any other temperature f, it may be had by multiplying the preceding expression by $\frac{f + 448}{660}$, which will make it $\frac{(f + 448)\,.\,\tau'}{48\,.\,(F' + 448)}$.

The atmosphere at 60° being 1, the specific gravity of vapour at 32° $= \frac{508 \times \frac{1}{5}}{48 \times 480} = \cdot 00441$, which exceeds ·00314 given by Dr. Thomson in the ratio of 8 to 11. In cases far above 212°, the difference will be much greater.

J. Herapath.

Article VIII.

On Right Angled Spherical Triangles. By Mr. James Adams.

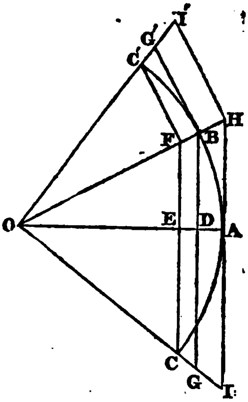
(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, near Plymouth, March 8, 1822.*

If, in your opinion, the following demonstrations of the theorems relating to right-angled spherical triangles independent of supplementary triangles, merit a place in the *Annals of Philosophy*, their insertion therein will much oblige,

Your humble servant,
James Adams.

Right Angled Spherical Triangles.



1. From any point E in the straight line O A, draw C E F at right angles to O A, and make E C the *hypothenuse,* and E F the *base* of a right angled plane triangle. Join O C and O F; then draw F C' perpendicular to O F, and equal to the *perpendicular* of the said triangle, and join O C', which will be equal to O C; for $OE^2 + EC^2 = OC^2$, $OE^2 + EF^2 = OF^2$, and $OF^2 + FC^2 = (OC')^2 = OE^2 + EF^2 + (FC')^2 = OE^2 + EC^2$; therefore, $OC' = OC$.

2. With the radius OC or OC', and centre O, describe the arc C A B C', intersecting O E and O F produced, in A and B; draw H A I and B D G parallel to C E F, intersecting O B, O A, O C, in the points H, I, D, G; also draw B G' and H I' parallel to F C', intersecting O C' produced in G' and I', then will E C, E O, A I, O I; D B, D O, A H, O H; F C', F O, B G', O G'; represent the sines, cosines, tangents, and secants, of the arcs C A, A B, and B C', respectively, or of their corresponding plane angles at O.

3. Conceive the planes O H I' and O A I to be folded in the lines O H and O A, so that the points C and C', or G and G', or I and I', coincide when raised up, which they can be made to do, because O C is equal to O C' (art. 1); in like manner O G is equal to O G', and O I to O I'; then a spherical triangle A B C will be formed whose sides A B, B C, C A, will have the common radius O A and centre O.

4. Now since three straight lines C E, E F, F C, meet one another, they are in the same plane E F C (2 11 *e*), therefore, and by construction, C F is at right angles to E F and O F, whence C F is at right angles to the plane O A H (4 11 *e*); but C F is in the plane O H I'; therefore, O H I' is at right angles to the plane O A H (18 11 *e*); hence the spherical angle A B C is a right angle (def. 6 11 *e*).

5. Since E C, E F, are at right angles to O A, the common intersection of the planes O A H and O A I, the angle C E F is equal to the spheric angle B A C (ibid).

6. The straight lines D G, D B, B G', and A I, A H, H I', being parallel to the straight lines E C, E F, F C', respectively, the plane triangles G D B, I A H, C E F, are similar (10 11 *e*).

The preceding construction, &c. being understood, the student will not find any difficulty in what follows:

7. In the right angled triangle C E F, we have rad. : E C :: sin. F E C : F C; that is, rad. : sin. A C :: sin. B A C : sin. B C; therefore, rad. sin. B C = sin. B A C sin. A C.

8. In the right angled triangle I A H, we have rad. : A I ::

cos. H A I : A H; that is, rad. : tang. A C :: cos. B A C : tan. A B, or rad. : tan. A C :: cot. A C : rad. :: cos. B A C : tan. A B, therefore, rad. cos. B A C = cot. A C tan. A B.

9. In the right angled triangle G D B, we have rad. : tan. B D G :: D B : B G; that is, rad. : tan. B A C :: sin. A B : tan. B C, or rad. : tan. B A C :: cot. B A C : rad. :: sin. A B : tan. B C; therefore, rad. sin. A B = tan. B C cot. B A C.

10. In the similar right angled triangles O B D, O F E, we have O B : O D :: O F : O E; that is, rad. : cos. A B :: cos. B C : cos. A C; therefore, rad. cos. A C = cos. A B cos B C.

In like manner, if the angle B C A enter into the process, we shall have

11. Rad. sin. A B = sin. B C A sin. A C.
12. Rad. cos. B C A = cot. A C tan. B C.
13. Rad. sin. B C = cot. B C A tan. A B.

14. By art. 8, cos. B A C = $\frac{\text{tan. A B cot. A C}}{\text{rad.}} = \frac{\text{tan. A B cos. A C}}{\text{rad. sin. A C}}$; by art. 11, sin. B C A = $\frac{\text{rad. sin. A B}}{\text{sin. A C}} = \frac{\text{rad. cos. A B}}{\text{sin. A C cot. A B}}$, therefore $\frac{\text{cos. B A C}}{\text{sin. B C A}} = \frac{\text{cos. A C}}{\text{cos. A B}} = \frac{\text{cos. B C}}{\text{rad.}}$, because (art. 10) cos. A C = $\frac{\text{cos. A B cos. B C}}{\text{rad.}}$: hence we have rad. cos. B A C = sin. B C A cos. B C.

15. By art. 9, cot. B A C = $\frac{\text{rad. sin. A B}}{\text{tan. B C}}$; by art. 13, cot. B C A = $\frac{\text{rad. sin. B C}}{\text{tan. A B}}$; therefore tan. B C A = $\frac{\text{rad.}^2}{\text{cot. B C A}} = \frac{\text{rad. tan. A B}}{\text{sin. B C}}$, then will $\frac{\text{tan. B C A}}{\text{cot. B A C}} = \frac{\text{tan. A B tan. B C}}{\text{sin. A B sin. B C}} = \frac{\text{rad.}^2}{\text{cos. A B cos. B C}}$, but by art. 10, cos. A B cos. B C = rad. cos. A C; therefore, $\frac{\text{tan. B C A}}{\text{cot. B A C}} = \frac{\text{rad.}}{\text{cos. A C}}$, from whence, rad. ; tan. B C A :: cot. B C A : rad. :: cos. A C : cot. B A C; therefore, rad. cos. A C = cot. B C A cot. B A C.

16. The reverse of art. 14 will give rad. cos. B C A = sin. B A C cos. A B.

17. By comparing the articles 9 and 11, 7 and 13, 10 and 15, 8 and 14, 12 and 16, and making radius unity, we have the following equations:

$$\text{sin. AB} = \left\{\begin{array}{l}\text{sin. BCA sin. AC}\\ \text{cot. BAC tan. BC}\end{array}\right\}$$

$$\text{sin. BC} = \left\{\begin{array}{l}\text{sin. BAC sin. AC}\\ \text{cot. BCA tan. AB}\end{array}\right\}$$

$$\text{cos. AC} = \left\{\begin{array}{l}\text{cos. AB cos. BC}\\ \text{cot. BAC cot. BCA}\end{array}\right\}$$

$$\text{cos. BAC} = \left\{\begin{array}{l}\text{sin. BCA cos. BC}\\ \text{cot. AC tan AB}\end{array}\right\}$$

$$\text{cos. BCA} = \left\{\begin{array}{l}\text{sin. BAC cos. AB}\\ \text{cot. AC tan. BC}\end{array}\right\}$$

18. The above equations, 10 in number, contain all the combinations that can be made out of the sides and angles of the right angled spherical triangle A B C, viz. A B, B C, A C, B A C, and B C A, which may be arranged in the following manner:

AB AC BCA	AB BC BAC	BC AC BAC	BC AB BCA	AC AB BC	AC BAC BCA	BC BAC BCA	AB AC BAC	AB BAC BCA	AC BC BCA

19. Baron Napier's two well known rules relative to right angled spherical triangles are comprised in the equations, art. 17; the left hand side of each represents the *middle part*, the right hand side composed of tangents and cotangents; the *adjacent parts*, and those containing sines and cosines, the *separated parts.*

ARTICLE IX.

On the Mathematical Principles of Chemical Philosophy.
By the Rev. J. B. Emmett.

(*Continued from* vol. i. p. 88, New Series.)

In the former papers, in the numbers for August, September, November, 1820, and January, 1821, I have explained the cause of the expansion of solid matter, cohesion, and crystallization, on the supposition that the force of attraction which is concerned, is the same as that which produces planetary motion, and that caloric is real matter. Before proceeding to the consideration of liquid and gaseous bodies, the atomic theory, and chemical action, I shall demonstrate that there is not in matter any force, except that which varies inversely as the square of the distance, and that the effects of heat cannot possibly arise from any motion existing among the particles of matter. I shall, therefore, commence with the consideration of the action of corpuscular forces in general.

There is a remarkable difference between the action of corpuscular forces and of those which act between sensible masses of matter; for example, a large mass of glass exerts no sensible force upon bodies which are placed very near to it, provided there be a sensible distance between them; but take a capillary tube of the thinnest possible glass, and immerse one end of it in water, the fluid will rise to, and remain at a certain height, which altitude is found, by experiment, to be inversely as the diameter of the tube, exactly or very nearly: hence there is a superficial attraction between the glass and the fluid, which very much exceeds the weight of water. That this attraction is entirely superficial is evident: take a thick tube, having the same opening, and the water will rise no higher.

A steel wire of 1-10th inch in diameter will lift a weight of several hundred pounds without breaking; therefore the attraction between two circular discs of steel of 1-10th inch in diameter exceeds by many million times the weight of a disc, having the same diameter, and a thickness equal to the diameter of one particle of steel; yet the attraction of two large masses of steel is almost insensible, if there be any sensible distance between them. These have been supposed to arise from a force which varies inversely as the cube or fourth power of the distance; such a force does not exist, as will appear from the following reasoning, which depends upon four phenomena.

Phenomenon 1. Place together the surfaces of two perfectly plain plates of glass; on separating them, their mutual attraction will be very sensible, even if a single fibre of silk be interposed; it acts in vacuo, or in the air.

2. A drop of any liquid will adhere to the under surface of a horizontal plate of glass or metal.

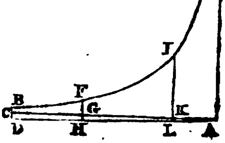
3. Two drops of any liquid, as mercury, water, &c. placed upon a horizontal plate of glass or metal, and very near to each other, approach and unite into one drop.

4. When two gases which do not combine chemically, as carbonic acid and hydrogen, are placed in a vessel in the order of their specific gravities, they soon attain a state of perfectly equable mixture.

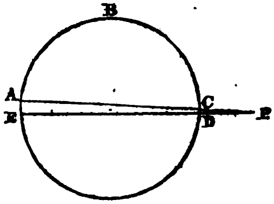
From phenomenon 1, solids exert upon each other a sensible force of attraction, when their surfaces are placed in contact with each other, which vanishes when the distance becomes sensible. Since all liquids expand by heat, and contract by cold, and since the quantity of the expansion is sensible compared with the entire volume, the particles of any liquid cannot touch each other at the ordinary temperature of the air, but are at a distance which bears a finite ratio to their diameters: at that distance, phenomena 2 and 3, their mutual attraction very greatly exceeds their weight, and from 3 is sensible at measurable distances.

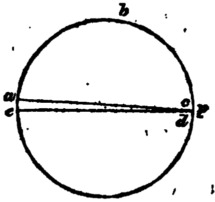
From phenomenon 4, the mutual tendency of gaseous particles to each other exceeds their weight at a distance which is many times greater than their diameter.

Suppose these effects to result from a force which is inversely as the cube of the distance. Take an evanescent or elementary pyramid C A D, whose vertex is A; let a corpuscle be placed at A; take two sections K L, G H, parallel to each other; and let these sections be evanescent plates of matter of equal

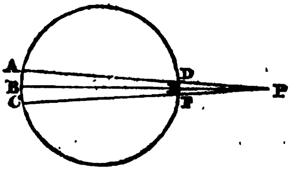


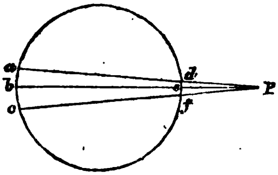
thickness. The area K L : area G H :: A L^2 : A H^2 but force of a particle at L : force of a particle at H :: A H^3 : A L^3; compound these proportions, and the force of K L : force of G H :: A G : A L. At A erect a perpendicular A N; take L I : FH :: force of plate K L : that of G H; and draw the hyperbola M I B, whose ordinates L I, H F, D B will be as the force of attraction of the sections at those distances; therefore the area B I L D will represent the force of the frustum C K L D; consequently the force of the whole pyramid to a particle placed upon the vertex is infinite, since the area B O M N A C is infinite.





Take now two equal spheres of the same matter, A B C D, and *a b c d;* let P be a corpuscle at a certain distance from A B C D, *p* one in contact with *a b c d;* take two elementary cones passing through the centres and equiangular, A P E, *a p e;* let A C = C K of the former figure; and in the same, let *a p* = G A; then the force with which P is attracted : force with which *p* is attracted : area B I K C : area F M N A G :: a finite area : an infinite one; therefore the force with which a corpuscle is attracted will be infinitely greater in contact than at the least possible distance. Therefore if the attraction be finite in contact, it vanishes when the corpuscle is removed to the least possible distance from contact; but if at any distance it be finite, it must be absolutely, and without any exception, infinite in contact. By observation, the force of cohesion is finite; but by phenomena 2, 3, and 4, the attraction at a distance is finite; therefore if the force be inversely as the cube of the distance, it must be infinite, which is not the case. But such a force does not exist.

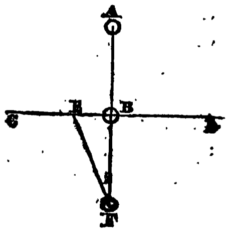


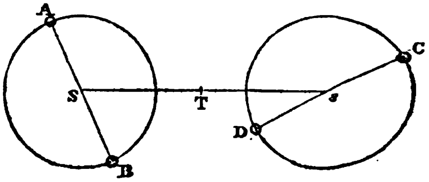


Let A B C, *a b c* be two unequal spheres, having the same density, the force of attraction being inversely as the cube of the distance; let P and *p* be corpuscles similarly situated with respect to each; in A B C, take any two elementary pyramids A D E B, B E F C; and in *a b c* take two respectively similar and similarly situated ones *a d e b*, *b e f c;* in each of the solid figures A D E B, *a d e b*, the number of similarly situated particles or points is as the cube of the homologous lines; that is, the number in A D E B : that in *a d e b* :: $PE^3 : pe^3$ or as $R^3 : r^3$ (R and *r* being the radii of the circles), but the force of each is as $pe^3 : PE^3$; therefore, by compounding the proportions, the force of A D E B : that of *a d e b* :: $R^3\, r^3 : r^3\, R^3 :: 1 : 1$; and the entire solids may be divided into an equal number of similar and similarly situated elementary pyramids, each of the corresponding pyramids of one attracting equally with one of the other; therefore, the entire attraction of the two spheres will be equal, when the corpuscles are placed at distances which are in proportion to their radii. Take, therefore, a globular vessel filled with a liquid, and of a large diameter, as one or two feet; find from the density of this body in its solid and fluid states, what ratio the distance between its particles has to their diameter (this is as $\sqrt[3]{\frac{1}{\text{Density}}}$) and place a particle of matter in this ratio from the globe; it will be equally attracted by it, as it would be if immersed in the liquid; and more, if nearer; but at this ratio of distance from the corpuscles themselves, the attraction exceeds the force of gravity by phenomena 2, 3, and 4; therefore it ought to be attracted to the globe, but no such effect takes place; therefore there is no such force. In addition, if there were, it would not be purely corpuscular, but would be affected by the mass; let there be two spheres of equal density, whose diameters are A and B, and the distance between their centres D; D being in a constant ratio to their diameters; the force of attraction will be as $\frac{A^3 \times B^3}{D^3}$ or as $\frac{A^6}{A^3}$ i. e. as A^3; therefore, if the force acting between two spheres of considerable magnitude be insensible, when there is a considerable distance between them, much more will it be insensible when the spheres are very much reduced, and the same ratio of distance remains. Form now a spherical drop of a liquid of particles of a given magnitude, and suppose its tendency to a plate of glass, phenomenon 2, to exceed its weight. Diminish the magnitude of the particles, and the tendency of each is as D^3, and the number in a section of a given magnitude is as $\frac{1}{D^2}$; therefore the whole tendency diminishes as D; since then at a considerable distance this force is almost insensible in large spheres, it cannot produce phenomenon 2. In the same manner, unless the force vary as $\frac{1}{D^5}$ at the least, it cannot produce phenomenon 2, 3, and 4, if it act by the entire mass.

The force, therefore, varying as the inverse cube or fourth power of the distance, does not exist; if it did, it would not be able to produce the observed effects, as it could not be purely corpuscular; but these forces are purely corpuscular, and depend not upon the mass of matter concerned, but merely upon the intensity of the attracting force, and the absolute distance of one attracting surface from another. This action will be considered in the next paper: in the remainder of the present communication, I shall proceed to an examination of the nature of that vibratory motion which has been supposed to produce the phenomena of heat.

In a former paper, I examined some parts of this hypothesis, and stated some reasons which induced me to adopt that which supposes heat to be real matter: I shall now demonstrate this intestine motion to be impossible. The particles of all matter are known to attract each other; and the direction of the force is invariably that of right lines meeting at the centre of the body, if spherical; and always meeting in it; therefore, when two bodies, whatever be their magnitude or figure, attract each other, they move in a right line which passes through them until they come into contact; after which, they remain at rest. Now if sulphuric acid and water be mixed, heat is excited, and the volume is diminished: the particles, therefore, are nearer to each other than they were before mixture. If the heat excited result from any vibratory motion, it must continue so long as the heat can be perceived; it, therefore, cannot be a motion which is in the direction of a right line joining the centres of the particles; for this brings them together, and produces a state of rest; the vibrations then must take place in a direction which is oblique to this line. Let us, therefore, see what sort of motion is possible. That one particle cannot oscillate about another as its centre, or about the centre of gravity of the two, is too evident to require any proof; but one may oscillate between two. Let A and F be two equal particles of matter placed at any given distance from each other; a third, B, may be made to oscillate between them in the right line C D, equidistant from them, and at right angles; let the force of attraction vary as $\frac{1}{D^n}$ (D being the distance, and *n* the index); but E and F, and E and A, and A and F, will always attract each other; let B be at E; the force with which it acts upon A and F to bring them together, or in the direction A F, is as $\frac{BF}{FE^{n+1}}$; while A and F attract each other in the same direction, force being as $\frac{1}{AF^n}$; therefore in a medium perfectly free from all resistance, the motion must soon cease,

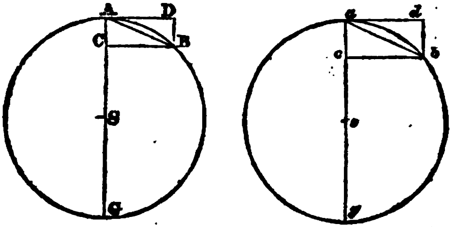




since the particles will soon come into contact, and of course preserve a state of rest. And if more of these systems be added, and systems themselves be made to oscillate, a state of rest will be attained very speedily and sooner in a large mass than in a smaller; but a large body retains its heat longer than a smaller one of the same kind of matter. The only motion that can be permanent is one of revolution. Let two particles A and B, equal and similar, revolve in a non-resisting medium; their revolution round S the common centre of gravity, and the centre of their common orbit may be continued indefinitely. Add another system, equal and similar, revolving round their common centre of gravity *s*; join S *s*, and bisect it in T. The motion of each system might continue independently of the other; but each particle of one being attracted by each of the other, the two equal systems must approach each other in the direction S *s*, and soon destroy each other's motion, except they revolve round T their common centre of gravity: add more systems, and they must all revolve round the common centre of gravity of the whole. If, therefore, any body be not at the absolute zero of temperature, there must be a motion of the whole of its parts round its centre of gravity, which in solids is impossible, and in liquids and gases would be evident by vortices which of necessity must produce mechanical effects, which is not the case. Besides this, the motion could only exist in a non-resisting medium; under the pressure of the air, and the weight of the body, it must be instantly destroyed. A solid could not retain its heat for many hours, since all must attain a state of rest in nearly the same time. This motion is also physically impossible, if the force of attraction be supposed to vary inversely as the cube of the distance; for if a body revolve round a centre of force, and be so attracted, its orbit must be a circle, or a logarithmic spiral, the former only being permanent, and revolving in a circle, if it be disturbed by any other force, which it must be by the attraction of adjacent particles, the weight of the mass, and pressure of the air, it becomes a logarithmic spiral approaching to the pole; and attaining this, all motion ceases; and when this is the case, the body becomes absolutely cold. In addition, whatever be the force of attraction, since cohesion vanishes when the particles of a body are separated to the least possible distance from contact, it is absolutely impossible for there to be any cohesion in solids if the particles have any such motion, since they never can touch each other. A motion of this sort, therefore, cannot continue; and it will cease in nearly the same space of time in all masses of the same matter. The motion itself may be now proved to be impossible, and, if it did exist, to produce effects quite the reverse of those which are observed in the phenomena of heat.

From what has been said, it is evident that were there no resistance to this species of motion, it could be permanent only when the force of attraction is inversely as the square of the distance; but in a resisting medium; that is, under the pressure of the atmosphere, and the weight of the parts of the body themselves, the motion must cease after very few revolutions. Consider the weight of the particles alone; this will give them a tendency to describe a parabola in the higher part of their orbit; then in the lower, the action of gravity counteracts the tangential force, and ultimately must destroy it; and even in liquids or gases, it is evident that there is resistance enough almost instantly to destroy all motion; and as the quantity of resistance thus opposed to the motion of the particles does not depend upon the volume of the liquid, but upon its density, the magnitude, and velocity, of the particles. If two unequal volumes of the same liquid contained in similar vessels be heated to the same temperature, the motion will be destroyed, or the heat will cease, at the same moment in each; but by experiment, the heat continues longer in the large than in the small volume. Again: since the force of attraction acts only in right lines, which in spheres are directed to their centres, it is quite impossible upon any principle that it can give rise to any sort of rotation whatever; since this can be produced only by a force acting obliquely to that of attraction; for any number of bodies attracting each other, and at liberty to move, will move towards their common centre of gravity only: there is, therefore, no cause whatever to give rise to any motion of rotation.

But even supposing the motion itself possible, the effects will not coincide with the phenomena of nature. The great phenomenon to be explained is this: All bodies, whether solid, liquid, or aeriform, are increased in volume by increased heat, and diminished by reduction of temperature; therefore, the spaces through which the particles move are greater at a high than at a low temperature, and the velocities are supposed to be quicker. Let us see how far this can result from the known laws of curvilinear motion. Let two bodies A and *a* describe respectively two circles A B G, *a b g*, with uniform motions; the centres of force being the centres S and *s* of the circles: let AB, *a b*, be two evanescent arcs, which are described in the same portion of time; from B and *b* draw the perpendiculars



BC, *bc*, and complete the parallelograms CD, *cd*; join AB, *ab*. By the nature of centripetal force, it is in each as DB : *db*.

Now AC : chord AB :: chord AB : AG ∴ $AC = \frac{\text{chord}^2 \cdot AB}{AG}$, similarly $ac = \frac{\text{chord}^2\, ab}{ag}$.

Since the evanescent chord is equal to its arc, the centripetal force with which A tends to S : that with which *a* tends to *s* :: $\frac{AB^2}{ag} : \frac{ab^2}{ag}$.

Again, since the arcs described in the same time are as the velocities V and *v*, centripetal force at A : that at *a* :: $\frac{v^2}{AG} : \frac{v^2}{ag}$ or :: $\frac{v^2}{R} : \frac{v^2}{r}$.

If T and *t* be the periodic times, centripetal force of A (F) : centripetal force of *a* (*f*) :: $\frac{R}{T^2} : \frac{r}{t^2}$.

Hence if the times T and *t* are respectively as R^n and r^n, the velocities are as $\frac{1}{R^{n-1}}$ and $\frac{1}{r^{n-1}}$ or V and $\frac{1}{R^{n-1}}$ and the centripetal force F is as $\frac{1}{R^{2n-1}}$.

If, therefore, the velocity of a particle be increased by heat, it is greater at a remote than at a nearer distance from its centre of force; and in this case $\frac{1}{R^{n-1}}$ must have a negative index, or 2 *n* must be less than 1; therefore $\frac{1}{R^{2n-1}}$ must be as 1 at the least, or centripetal force must increase with increased distance. If such a force as this existed in the particles of matter, its effects upon the moon would be very great, as may be proved by phenomena 2, 3, and 4. If the force of gravity vary inversely as the square or cube of the distance, the greater velocity attains in the smaller orbits; which is highly inconsistent with the phenomena of the solidification of liquids by cold. And in all cases, if the motion be obstructed, i. e. if part of the centripetal force be removed, the consequence is a diminution of the orbit, which is changed into a spiral, in which all motion soon ceases, as the bodies ultimately come into contact. If, therefore, this were the cause of heat, the pressure of the atmosphere would soon destroy the motion, and the body would arrive at the true zero of temperature.

The phenomena of combustion furnish us with another argument which is fatal to this hypothesis. Warm a grain of sand, not to ignition, and let it fall upon a piece of dry phosphorus; it will set it on fire, and the whole will be consumed; intense heat will be liberated: this may communicate heat to larger masses of combustible matter, and produce a fire of unlimited magnitude. If this be the result of any motion, the whole must have been communicated from the single grain of sand, for from the union of the particles of the combustible body with those of oxygen, no such motion can possibly result; and every person who is at all acquainted with but the first principles of mechanics must see that such an effect is utterly impossible, and absolutely opposed to every known law of the communication of motion, it being quite impossible that the motion given to a small mass of matter can communicate a greater in greater masses, which must be the case, if the cause of heat be motion. Still more impossible is the existence of a mere vibratory or undulatory motion.

In the above researches, perhaps, I may not have described the sort of motion which is intended; the reason why I have selected that of rotation is, because it is the only one which can have any permanency, and has been more particularly defined by some writers, than either vibration, undulation, or any other that has been supposed; and in general, the intestine motions that have been introduced to explain the phenomena of heat have been expressed in such vague terms, and in a manner so totally destitute of precise definition, that it is impossible to collect any thing that will enable any one to submit it to a mathematical inquiry. I have, therefore, selected that which of all others is the most likely to answer the conditions, being the only one that can be permanent, when there is no resistance opposed to it.

In the next, I shall apply the principles to the constitution of liquids.

(*To be continued.*)

ARTICLE X.

On Diaspore. By G. B. Sowerby, FLS.

(To the Editor of the *Annals of Philosophy.*)

SIR,

A SECOND mass of this curious and rare mineral has just fallen into my hands: it is a remarkable circumstance that the locality of a mineral so extremely singular, and at the same time so well characterized, should have remained for so long a period unknown; for it will be recollected that the only mass hitherto known had come accidentally into the hands of M. Lelievre, and that from this specimen, whatever small bits exist in collections have been broken. From the data which I have with this specimen, I think I shall be able to trace its precise locality.

Mr. Children has been so obliging as to subject some fragments to a critical examination by the blowpipe; and he has favoured me with the following account of his experiments. He will probably take advantage of this opportunity to complete a new analysis of this mineral, which he will communicate to you.

I am, Sir, yours, &c.

G. B. Sowerby.

Examination of the above by means of the Blowpipe. By J. G. Children, FRS. L. & E. FLS. &c. &c.

Specific gravity = 3·205, which is probably a little too light, as some very minute air bubbles adhered to the specimen after immersion in water which I could not completely detach from it.

Alone in a glass tube decrepitates violently, splitting into minute fragments with a sudden explosion. At first but little moisture is given off, but when the bottom of the tube is nearly red-hot, abundance of water condenses in the upper part. The water has no effect on blue litmus paper. The assay loses its colour, more or less, by heat; and frequently becomes milk-white on the surface: the fragments do not brown moistened turmeric paper.

Alone in the forceps, or *on charcoal*, it is infusible.

With soda, on the platina wire, it gives an opaque globule of a dirty pearl colour, inclining to yellow, in the oxidating flame. In the reducing flame the globule is almost black externally; internally dark-grey, inclining to brown. The assay in this experiment was pulverized; soda has scarcely any action on a fragment.

With borax, on the platina wire, in the oxidating flame, a fragment of the assay dissolved with difficulty in a large proportion of the flux into a perfectly transparent glass, which was yellow, while hot; quite colourless, cold. In the reducing flame, the glass retained its transparency, and was colourless both hot and cold.

With salt of phosphorus, on platina wire, in the oxidating flame, the pulverized assay dissolves slowly into a perfectly transparent glass, which is deep-yellow, while hot, quite colourless cold. In the reducing flame, the glass is colourless both hot and cold. No silica skeleton nor residuum is left, nor does the glass become opaque by flaming with either of the two last fluxes.

With nitrate of cobalt, a fine deep-blue colour.

With boracic acid and iron, no trace of phosphoric acid.

Berzelius states that the fragments of a small piece of Lelelievre's diaspore, "heated to slight redness," restore the blue colour of reddened litmus paper. (Use of the Blowpipe, p. 227.) I did not, however, find this to be the case, and therefore Mr. Sowerby's mineral perfectly agrees in all its *blowpipe characters* with the diaspore of Lelievre.

ARTICLE XI.

On Cadmium, and the Sources of procuring it in Quantity.
By Mr. W. Herapath.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Bristol*, 56, *Old Market-street*, *April* 1, 1822.

WHEN reasoning on the properties of cadmium, its volatility in the metallic state, and fixidity as an oxide, I expected to meet with it in the sublimed products of the zinc smelting house. I accordingly visited one in this neighbourhood, and brought away some specimens, among which I have discovered the metal in much larger quantity than it has hitherto been obtained, varying from 12 to 20 per cent. being six times more plentiful than in the richest substances examined by Stromeyer.

It may be proper here to mention the exact situation in which it is to be met with. I believe it is well known that zinc is reduced from the ore by a sort of distillation, the calamine with small coal as a flux being introduced into a pot closely covered on the top, but having a tube leading from its bottom into a vault below; just under this, there is a vessel of water placed, and a moveable tube is kept long enough to reach from the short tube nearly to the surface of the water.

The workmen are not in the habit of connecting the two tubes until what they call the "brown blaze" is over, and the "blue blaze" begun: this brown flame is owing to cadmium absorbing oxygen; it sublimes, and is attached to the roof of the vault, but in the greatest quantity immediately over the orifice from which it issues; it is mixed with soot, sulphuret of cadmium, and oxide of zinc. The colour is a compound of brown, yellow, black, and white, varying with the quantities of the different substances which enter into the mixture.

To obtain the metal, I have used the following process: Add to the sublimate an excess of muriatic acid; filter and wash the residue; add the washings to the liquid; evaporate to dryness to get rid of the excess of acid; redissolve in as little water as possible, filter again to separate the insoluble part; introduce a plate of zinc, and the cadmium is precipitated in the form of small leaves. In reducing these to a mass without loss, I have found considerable difficulty from the volatile nature of the metal, &c. I followed Stromeyer's process, until I found that the globules which sublimed into the cold part of the tube were more malleable, and did not, as he describes, scale off when long hammered; I, therefore, put the spongy precipitate into a black glass tube (closed at one end), together with a little lamp-black or wax; and kept the end containing the metal in the red heat of a common fire-grate until the whole of the cadmium was sublimed into a part of the tube very near where it was red-hot. After throwing out what remains in the bottom (and which may be done without danger of losing any of the sublimate, as that adheres very firmly to the glass), introduce some wax, and heat it gently; while the wax is in the act of burning, the metal will melt and form a button, if assisted with slight agitation; it should be allowed to cool before it is taken out. Its colour is such that those of my friends who have seen it supposed it to be silver; but when compared with a piece of that metal it has a blue cast. The bottom and sides of the button are covered with facets, having exactly the appearance that it would assume if it had been struck on every part of the surface with a small hammer. When examined with a strong lens, the superficial crystals resemble stars, each having a nucleus from which six spiculæ radiate. I have inclosed a lamina for your examination; you will perceive that it is more malleable than Stromeyer found it; it is very probable that his contained a little zinc. The specific gravity at 62° of the specimen from which this piece was cut, was 8·677.

As to the weight of the atom, Stromeyer calls it 6·9677, but his analysis gives

Carbonate	7·05
Sulphate	7·05
Nitrate	6·93
Chloride	7·15
Phosphate	6·89
Oxide	6·96
Average	7·005

From this, I think, we might infer, that the true number is 7, at least in the absence of a greater number of experiments. He has stated, *Annals*, vol. xiv. p. 271, that "the precipitate formed from muriate of cadmium by carbonate of ammonia is insoluble in an excess of carbonate." This has been contradicted by the late Prof. Clarke, *Annals* for March, p. 196. In order to set the question at rest, I made some muriate of cadmium, both of the substances *being pure;* then put 1-10th grain into two watch glasses, adding a few drops of water to dissolve it. I poured upon it a solution of carbonate of ammonia (saturated at 60°) until it amounted to 227·17 gr. it appeared to have dissolved a part; but having stood 12 hours, it was decanted, washed, and kept at 150° long enough to evaporate any carbonate of ammonia left in it; the residue was ·08. Now as ·1 grain of muriate of cadmium is equal to ·084 carbonate, supposing the ·004 of loss to be dissolved, instead of the unavoidable errors of experiment, it cannot be more soluble than one part in 56792, so near approaching to insolubility as to lead to the conclusion that Dr. Clarke was in error.

In one instance when subliming the pure metal, the top of the glass tube was not closed, and it had been kept in the fire longer than usual; upon scraping out the sublimate, instead of metal, it consisted of purple, opaque, radiating, needle formed crystals; imagining it to be a carburet, I threw it into muriatic acid, where it dissolved without effervescence, and left no residue; it, therefore, must have been a crystallized oxide, but whether the oxygen was in a different proportion than hitherto found, I have not had time to ascertain; but as I am engaged at present in experiments upon the metal, in a short time you will receive an account of any thing which may be interesting. I cannot conclude without offering an opinion as to the best mode of procuring it in sufficient quantity to be useful in the arts. As the cadmium rises earlier than the zinc, the first products of the distillation must contain more than the last; if the tube was put up immediately upon the pot being charged, and the first few pounds of zinc kept separate, I have no doubt but the zinc smelter would find in it enough to pay his expenses in subliming it; in fact, the additional expense would be very trifling, as they do not sell the zinc in the crude state in which it is found after distillation, but always melt it into lumps; this is done in an iron pot, by putting an air-tight top to it, and increasing the heat, perhaps, 200°, which may be done with little fuel, they would accomplish it, and thus be enabled to render the new metal at a price very little higher than they do zinc. I remain, Sir,

Your most obedient servant,

William Herapath.

Article XII.

On the Method of analyzing the Ores of Nickel, and on a new Combination of Nickel with Arsenic and Sulphur. By J. Berzelius.

(*Concluded from* p. 216.)

III. *Analysis of a White Ore of Nickel from Loos, in Nelsingland.*

This ore of nickel is not crystallized; it is a white brilliant metallic mass, of a granular structure. There are two varieties which are very difficultly distinguished from each other. In one of these varieties, the grains are rounder; it decrepitates in the fire with extreme violence; when heated in a glass tube closed at one end, it leaves a mass resembling kupfernickel, and a portion of sulphuret of arsenic sublimes. The other variety is also granular, but the grains are less equal; the structure is by this rendered less compact; it has then the appearance of a cobalt ore, it decrepitates less, gives sulphuret of arsenic by distillation, and leaves a silver-white residuum.

It appears that it is the first of these varieties which was examined by M. Psaff. The two first analyses which I am now going to describe were made on the first variety, and the remainder on the second, as may be seen by the results.

(A.) *Analysis by Means of Nitric Acid.*

a. Forty parts of the pulverized mineral were treated with pure nitric acid until the undissolved portion appeared to be merely sulphur. The residuum weighed 1·38 part. The sulphur was burnt, and left 0·27 part of silica, or at least of an earthy powder. The weight of the burnt sulphur was, therefore, 1·11 part.

b. The solution precipitated by muriate of barytes gave 28·77 parts of sulphate of barytes, equivalent to 3·96 of sulphur, which, added to 1·11 before obtained, make a total of 5·07 parts, or 12·675 per cent.

c. The barytes added in excess was separated by sulphuric acid; and afterwards a current of sulphuretted hydrogen gas was passed through the solution, as long a sulphuret of arsenic was formed, which was washed upon a weighed filter; it was well dried, and was weighed while inclosed in a covered platina crucible, in order to prevent the attraction of moisture during its cooling. It weighed 36·87 parts. It was afterwards treated with caustic ammonia upon the same filter; the ammonia dissolved the sulphuret of arsenic, leaving as a residuum the sulphur which was separated from the sulphuretted hydrogen during the experiment by atmospheric air, and the peroxide of iron of the liquid, which was reduced to protoxide. This sulphur was white, and weighed, when well dried, 1·17 part. The 36·87 parts, therefore, contained only 35·7 parts of sulphuret of arsenic, equivalent to 21·75 parts of metallic arsenic, or 54·38 per cent. of the weight of the mineral.

d. The solution deprived of arsenic, and heated to reoxidize the protoxide of iron, ammonia was afterwards added to it in great excess. It occasioned a precipitate of an olive-green colour; this was separated, and dissolved in muriatic acid: this solution was neutralized as nearly as possible, and the iron was precipitated by succinate of ammonia. The succinate of iron decomposed by heat in an open vessel gave 1·83 part of oxide of iron, which, when treated with soda by the blowpipe, smelled strongly of arsenic.

e. The ammoniacal solution was mixed with that from which the iron had been precipitated by the succinate of ammonia; subcarbonate of potash was poured into it, and the ammonia was evaporated. The oxide of nickel thus separated weighed 15·32 parts.

f. This oxide was treated with muriatic acid, and the solution evaporated to dryness; the dry mass was redissolved in water, which left a white powder undissolved. This powder was arseniate of peroxide of iron, which, after heating, weighed 1 part, equivalent to 0·44 part of metallic arsenic, and to 0·215 of iron. The quantity of oxide of nickel obtained was, therefore, 14·32 parts, equivalent to 11·27 of metallic nickel. In that which I here estimate as pure nickel, there was a small quantity of lime which became evident when the solution of neutral muriate of nickel was mixed with an excess of subcarbonate of ammonia; but the quantity of carbonate of lime precipitated was too inconsiderable to be separated and weighed.

The analysis then gives

Arsenic, *e f*	55·50
Sulphur, *a b*	12·67
Nickel, *f*	28·17
Iron, *d f*	3·63
Earthy matter	0·61
	100·58

The excess given by this analysis is undoubtedly owing to the hygrometric moisture contained in the sulphuret of arsenic at the time of weighing. This inconvenience would be, however, easily avoided by fusing the sulphuret in vacuo, if at the time of the solution of the ore, the formation of a small quantity of arsenic by the action of the nitric acid upon the arseniuret could be prevented; this last circumstance renders the method employed in the analysis described quite inapplicable; for it is not a question of approximations, but of results, as accurate as possible.

(B.) *Analysis by Means of Nitromuriatic Acid.*

The lime found in the preceding analysis made me suspect a mixture of carbonate of lime with the ore. In the following analyses I have employed an ore of nickel which was powdered and digested in dilute muriatic acid, until all the carbonate of lime was extracted.

a. Ten parts of the nickel ore dissolved by nitromuriatic acid, left 0·55 part undissolved; this was sulphur, which burnt without residuum. The solution gave 6·5 parts of sulphate of barytes, equivalent to 0·897, or added to 0·55 14·4 per cent. of sulphur.

b. The liquid was then deprived of the excess of barytes by means of sulphuric acid. It was afterwards precipitated by caustic potash, added in great excess. The precipitate, well washed, was re-dissolved in muriatic acid, and the solution was mixed with caustic ammonia, until the oxide of nickel was dissolved. The ammonia left 0·98 part of sub-arseniate of iron. This dissolved in concentrated muriatic acid, without any green appearance, which indicates even very small quantities of nickel, or cobalt, when the acid is concentrated. This solution of the sub-arseniate was decomposed by caustic potash, leaving 0·82 part of sub-arseniate, which I have mentioned above. The potash had, therefore, taken up 0·16 part of arsenic acid. The 0·82 part of subarseniate, produced by the potash, contained 0·764 of oxide of iron, and consequently the 0·98 part are equivalent to 0·116 of metallic arsenic, and 0·529 of iron.

c. The ammoniacal liquid, mixed with the caustic potash, furnished 3·44 parts of oxide of nickel, equivalent to 2·7 parts of the metal. The liquid, separated from the oxide of nickel, was evaporated until the ammonia was completely volatilized, without depositing any thing. It was afterwards mixed with the alkaline liquid, resulting from the precipitation of the oxide of nickel, and the subarseniate of iron. This mixture now contained arsenic acid. It was neutralized by means of muriatic acid.

d. To separate the arsenic acid, I used M. Berthier's method. I dissolved ten parts of common iron wire in muriatic acid, and poured the solution into the preceding, and precipitated with an excess of ammonia. The precipitate, washed and heated to redness, weighed 22·4 parts. But ten parts of metallic iron, which in general contain 0·0005 of its weight of carbon, give 14·35 parts of peroxide. Subtracting 14·35 from 22·40, gives 8·05 parts for arsenic acid, which, added to the 0·116 part obtained in *b*, gives a result of 8·166 parts of arsenic acid, equivalent to 5·332 of metallic arsenic.

The analysis therefore gives

Arsenic	53·32
Sulphur	14·40
Nickel	27·00
Iron	5·29
	100·01

The difference between the two analyses is inconsiderable, and shows that the analytic methods agree very nearly: especially if we consider that the sulphur, determined by the same method in the two experiments, varies in proportion, and appears to indicate a difference of composition in the ores. Still as the arsenic acid could not be entirely separated from the peroxide of iron, the result is always subject to uncertainties, with respect to the relative quantities of the iron and arsenic.

(C.) *Analysis by Means of Nitromuriatic Acid and Acetate of Lead.*

This analysis, as well as the following, were made on pieces taken from a specimen coming also from Loos; but as I have said above, forming another variety of the nickel ore.

a. 15·1 parts of the pulverized ore (deprived of its carbonate of lime) were dissolved in nitromuriatic acid, which left 0·56 residuum, of which 0·4 were sulphur, and 0·16 silica.

b. The solution, precipitated by muriate of barytes, gave 18·18 parts of sulphate of barytes, equivalent to 2·5 parts of sulphur: the whole quantity of sulphur was, therefore, 2·9 that is to say, 19·29 hundredths of the weight of the ore.

c. The liquid having been filtered, was saturated with ammonia until a precipitate began to appear; acetate of lead was then poured in as long as a precipitate formed. This precipitate was then washed in boiling water, and as after an edulcoration continued for a long time, the washings continued to re-act with nitrate of silver. I dried it, without attempting to deprive it entirely of its muriate of lead. Heated in the fire, it became yellow, and weighed 45·5 parts.

I dissolved 42·53 of this in diluted nitric acid, which left undissolved 0·58 part of red oxide of iron, which gave out the odour of arsenic in a heated tube. The solution, mixed with sulphate of soda, gave 40·73 parts of sulphate of lead, and afterwards, with nitrate of silver, 3·77 parts of chloride of silver. The liquid was deprived of the superfluous nitrate of silver, by the addition of muriatic acid; and afterwards filtered and evaporated to dryness. The dried mass dissolved in water left a white powder, which weighed 1·25: this was arseniate of lead. The liquid neutralized as nearly as possible by means of caustic potash, deposited 0·1 part of a light and whitish substance; this was the neutral arseniate of iron. These quantities were obtained from the 42·53 parts dissolved by the nitric acid; calculating for the 45·5 parts, the entire weight of the precipitate obtained by means of the acetate of lead, we have the following quantities:

Sulphate of lead ..	43·58	=	32·06	parts of oxide of lead.
Chloride of silver ..	4·03	=	0·77	of muriatic acid.
Arseniate of lead ..	1·34	=	0·89	of oxide of lead.
Arseniate of iron ..	0·11	=	0·03	of oxide of iron.
Oxide of iron		=	0·62	
			34·37	

This 34·37 must be taken from the 45·5 parts of the above mentioned precipitate, to obtain the weight of the arsenic acid, which is 11·13, and which corresponds to 7·268 parts of arsenic.

We must not expect that a result obtained by so intricate an experiment can be exact; because, if the small errors of each of the determinations, for example, small losses, are added together, the sum will occasion a great error in the general result. And this really happens.

d. The liquid remaining after the separation of the arsenic acid from the acetate of lead, was deprived of the excess of lead by means of sulphuretted hydrogen gas, and the oxide of nickel by caustic potash. The oxide thus obtained weighed 5·92, which makes 30·8 hundredths of metallic nickel in the ore.

The general result of the analysis is, therefore, as follows:

Arsenic	48·06
Sulphur	19·29
Nickel	30·80
Iron	2·99
Silex	1·00
	102·15

As the result of so complicated an experiment cannot be satisfactory, I devised another analytical method, as follows:

(D.) *Analysis by Means of Chlorine.*

A portion of the pulverized ore, previously treated with dilute muriatic acid, to separate the carbonate of lime, was introduced into a bulb, blown in the middle of a piece of barometer tube; a current of chlorine gas, dried over fused chlorine of lime, was made to pass through the tube, and when nearly the whole of the atmospheric air was expelled, the bulb was heated by means of a spirit lamp. The metals and the sulphur combined with the chlorine gas; the mixture of the muriatic, arsenic, and hyposulphurous acids formed, distilled in small drops, and were collected in water. The less volatile chlorides remained in the bulb. The operation continued for 12 hours; but the evolution of chlorine gas was always very slow; 45·685 parts of the ore were employed.

(A.) *Examination of the Chlorides remaining in the Bulb.*

a. Water was introduced into the bulb; the yellowish mass did not appear to dissolve at first, and the water dissolved only muriate of iron; but in an hour, muriate of nickel was also dissolved, and the water left a residuum of 15·12 parts. which were ore unacted upon.* The gas had, therefore, decomposed 30·565 parts. The solution was mixed with nitric acid, and boiled, to convert the iron into peroxide. It was afterwards neutralized by ammonia, and the iron precipitated by succinate of ammonia. The succinate of iron, burnt upon a small porcelain capsule, left 1·82 part of peroxide of iron, which, treated with soda by the blowpipe, gave no trace of arsenical odour. This quantity of peroxide is equivalent to 1·26 of metallic iron, or 4·11 per cent. of the weight of the ore employed.

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* To satisfy myself that the portion undissolved by water was not altered, I examined it with a microscope, by which it appeared like small fragments, partially corroded. When dissolved in nitromuriatic acid, it gave, with sulphate of barytes, the same quantity of sulphur as the portion decomposed by chlorine.

b. The solution, freed from iron, was mixed with excess of ammonia. It became of a blue colour, without depositing any thing. The oxide of nickel was precipitated by caustic potash; it weighed 11·725 parts. It was dissolved by muriatic acid, and the solution evaporated to dryness; the dry matter, treated with water, left 0·1 of silica, undissolved; the weight of the oxide of nickel, was therefore, only 11·625, equal to 29·95 per cent. of metallic nickel in the ore. A current of sulphuretted hydrogen gas was passed through the solution of muriate of nickel, which occasioned a small precipitate, that was with difficulty determined by the blowpipe to be copper.

c. The solution precipitated in *b,* by caustic potash, had a rose coloured tint. It deposited, by evaporation, 0·37 of oxide of cobalt, equal to 0·92 per cent. of metallic cobalt in the ore. This cobalt contained a little copper, which I did not think it worth while to separate and weigh.

d. The solution which had deposited the oxide of cobalt, was supersaturated with muriatic acid, and evaporated to dryness. The saline mass, treated with water, left 0·18 of silica. As I employed caustic potash, which, when treated in the same manner, gave no trace of silica; this silica was derived from the nickel ore; but as chlorine gas does not combine with silica, that is to say, with the oxide of silicium ready formed, it is very probable that this silica existed in the ore as arseniuret, or sulphuret of silicium.

(B.) *Examination of the Volatile Substances.*

a. The liquor which distilled was of a very deep orange colour. When dropped into water, it became milky, and deposited sulphur. The excess of chlorine, continually absorbed by the water, gradually acidified the sulphur; so that towards the end of the operation very little of it remained. The bottle, which served as a receiver, had a ground glass stopper, and put in a place the temperature of which varied between 85° and 140° of Fahrenheit; after some hours' digestion, the stopper was taken out, and the liquor was heated to ebullition, in order to expel the excess of chlorine. The particles of sulphur which remained unacidified, gradually attracted each other, and at length formed a small globule, which, after the expulsion of the gas, was evaporated: it weighed 0·55 part.

b. The liquid was neutralized by caustic potash, without being rendered turbid; proving that no part of the chloride of iron had been volatilized. It was rendered slightly acid, and mixed with muriate of barytes; it gave 38·92 parts of sulphate of barytes, equivalent to 5·37 of sulphur, which, with the 0·55 obtained in *a,* makes a total of 5·92, or 19·34 per cent. of the weight of the ore.

c. The best method of obtaining the arsenic acid would undoubtedly have been to precipitate it with peroxide of iron;

but the bulky state of the subarseniate of iron, and the long washing which it requires, induced me once more to try the acetate of lead, in the hope that the absence of iron would render the composition of the precipitate less complicated. I, nevertheless, deceived myself; and I had afterwards to separate the lead by sulphuric acid, and the muriatic acid by nitrate of silver. The acetate of lead gave a precipitate, which weighed 85·85 parts. Treated with sulphuric acid, it gave 63·53 of oxide of lead, and with nitrate of silver 1·06 of muriatic acid, which, both deducted from 85·85, leave 21·26 for the arsenic acid, equivalent to 13·88 of metallic arsenic, or 45·37 per cent. of the weight of the ore.

The analysis therefore gave

Arsenic	45·37
Sulphur	19·34
Nickel	29·94
Cobalt, with a trace of copper	0·92
Iron	4·11
Silica	·90
	100·58

I shall not stay to inquire into the cause of the excess of weight given by the analysis; those who are accustomed to accurate researches, know how easy it is to fall into such an error, when every effort is made to lose nothing. I shall only add, that if the silica of the ore was in the state of silicium, there would scarcely be any excess.

As to the chemical constitution of the ore analyzed, it is evident that it is analogous to that of arsenical iron and grey cobalt; that is to say, that it contains an atom of quadrisulphuret of the metal combined with an atom of biarseniuret, of the same metal; calculating the composition on this view, we have the following proportions:

Arsenic	45·17
Sulphur	19·32
Nickel	35·51

This is the composition of a combination containing neither iron nor cobalt. But the three metals in question may occur, in a similar state of combination, mixed together in different proportions, without greatly influencing the proportions of arsenic or sulphur, because the saturating capacity of nickel and of cobalt are exactly similar, and that of iron very little exceeds it. Consequently, when in the ore, which I have analyzed, the weights of the cobalt and iron are added to that of the nickel, we have 34·95, which agrees very nearly with the calculated result. The variety analyzed on the two first experiments, and that examined by M. Psaff, were then a mixture of arsenical nickel with this last combination, which may be called grey nickel, to indicate that its composition is analogous to that of grey cobalt.

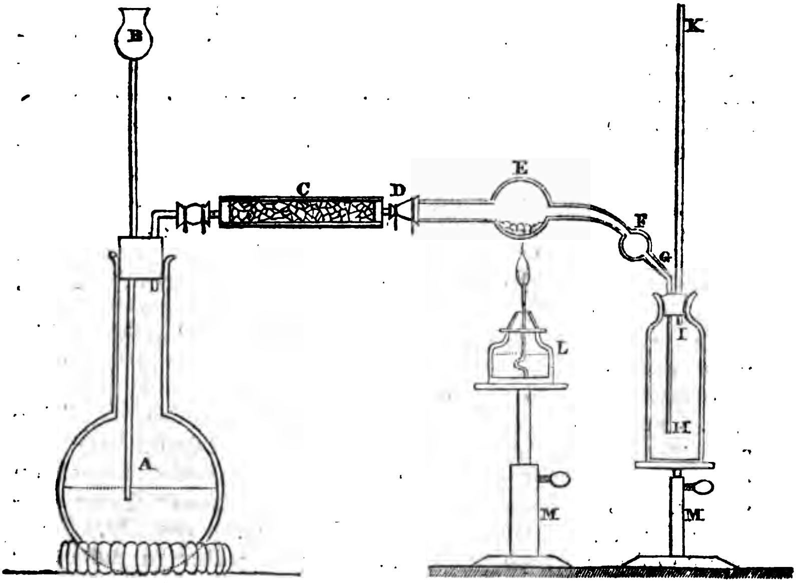
It is evident, that the quantity of arsenic found in the mineral is too small to form a biarseniuret with the whole of the nickel; and the variable quantity of iron indicates that the sulphuret of this metal, or its arsenio-sulphuret, is mixed with it, and not combined in definite proportion, either with the arseniuret of nickel, or the arsenio-sulphuret.

IV. *Detailed Account of the Method of analyzing the Arseniurets, or the Arsenio-sulphurets of Nickel and Cobalt, by Means of Chlorine Gas.*

It has been seen, by what has been stated, that arsenic and other metals were completely separated, only by the decomposition effected by means of chlorine gas; and notwithstanding each of the other methods has given an approximation to the true composition, they cannot be regarded as good; for every experiment which leaves the operator in doubt, must be considered as inconclusive, unless it be confirmed by another less questionable process; but then it is better immediately to employ the most certain method.

I am, therefore, now going to relate more particularly the method of analyzing nickel ores by chlorine gas, and I shall notice the precautions which are requisite to obtain the object.

At three inches from one of the ends of a barometer tube, blow a bulb of such a size that it shall be only one-third filled with the powder of the substance to be examined. On the other



side of the bulb, draw the tube out a little, and blow a second and smaller bulb, after which, bend the drawn tube, as shown by the figure D E F G H. The tube is to be weighed at first empty, and afterwards with the substance to be analyzed, in order to determine its weight.

To evolve the chlorine gas, a vessel, A, may be employed, capable of holding from two to four pints; a mixture of common salt and of oxide of manganese is to be put into it, and it is then to be two-thirds filled with water; the orifice is then to be closed with a cork, through which pass a long stemmed funnel, B, and a small bent tube, which gives vent to the gas.

Fig. A B D explains this arrangement better than any description. From the bent tube, the gas passes into another tube, C, which contains small fragments of fused chloride of calcium; and from this it passes into the small apparatus, which contains the powder to be analyzed.

The joinings are made by small tubes of caoutchouc, firmly tied round the glass tubes. The drawn tube, G H, descends perpendicularly into a bottle, H I, one-third filled with distilled water, G H passes through a cork, which closes the mouth of the bottle, and which contains also another tube, from 24 to 36 inches long, by which the excess of chlorine gas escapes, and by which it is conveyed out of the room by the chimney. This arrangement is represented in the fig. G H I K. The bottle is placed at a convenient height, by means of the screw, M.

When every thing is thus arranged, concentrated sulphuric acid is poured by the funnel B into the vessel, until a disengagement of gas begins to take place. Care must be taken that the mixture does not become too hot, as it would occasion too rapid an extrication of chlorine gas. The disengagement is sufficiently quick wehn four or five bubbles rise every minute out of the bottle H I.

As soon as the greater part of the atmospheric air is displaced by the chlorine gas, a spirit lamp is placed near the bulb E. A very small flame only is requisite, and too great a degree of heat must be avoided; for it is difficult, especially at the beginning, to expel the atmospheric air perfectly, and this might produce arsenious acid in a small part of the ore, which would render the result inaccurate.

As the mass becomes hot, an orange coloured fluid distills, which condenses in the little bulb F; and as this fills, it runs through the tube G H, and falls into the water.

The operation continues in this manner without requiring any attention, unless to add a small quantity of sulphuric acid every 2 or 3 hours, when the disengagement of gas goes on slowly. No artificial heat is employed to assist the extrication; for in that case it would take place too quickly. Each bubble of gas which passes through the water in the bottle

H I, gives a slight vapour, which is owing to a portion of the double acids which the water has not yet dissolved; but this vapour falls back again upon the surface of the water, and none of it is lost. If, on the other hand, the gas is extricated too quickly, the acid vapours will not have sufficient time to condense, neither in the bottle, nor the long tube I K, and a vapour will be seen to escape from the opening.

During the operation, permuriate of iron sublimes in small red transparent flakes, and a small quantity of which is even deposited, E D. On this account, it is proper to have this so long that the portion sublimed does not escape.

Another portion of the muriate is carried by the current in the direction E F. When the acids condense with the muriate, there results a white crystalline matter, a small quantity of which even descends into the small bulb F, which is made for the purpose of preventing the muriate of iron from descending into the bottle.

This white mass is the orange coloured liquid; and when this latter has been poured into the bottle, the white mass is to be decomposed by a slight heat, the double acids are volatilized, and the muriate of iron re-appears with its red colour.

The operation may be discontinued when judged convenient; I suffered it to continue 28 hours; but I found that in the last 12 hours that I had gained nothing. The chlorine gas does not produce a partial decomposition; the whole of the ore combines with it, and that which remains after the operation has undergone no alteration; it is, therefore, not at all necessary to wait until all is decomposed by the operation.

At the moment in which the operation is discontinued, a portion of the volatilized acids still adheres to the sides of the small apparatus, from the large bulb, to the opening of the tube, H. To get rid of it, both bulbs are heated at the same time; but to such a degree, as not to volatilize the muriate of iron; and whilst the bulbs are cooling, a solution of carbonate of potash is poured through the funnel, B: this occasions a rapid disengagement of carbonic acid gas, by which the last vapours of the acids are carried off.

When at length D E F G H is removed, C H is washed several times in cold water, to remove all traces of acid which may adhere to it, both inside and out, and this water is to be poured into the bottle. Afterwards the metallic chlorides are dissolved in water. The chloride of iron dissolves readily, but the chloride of nickel resists the water at first. A drop of muriatic acid ought to be added to the liquor to prevent its being turbid; after filtration, the undissolved portion is to be weighed.

This liquid contains some protomuriate of iron, mixed with permuriate. It is this muriate which is first formed, and which, usually enveloped in muriate of nickel, is prevented from combining with a fresh quantity of chlorine gas. It is, therefore, necessary to peroxidize the iron, by adding nitric acid, and heating it to ebullition. Afterwards it is saturated with ammonia, and the iron precipitated with succinate of ammonia; and at last, excess of ammonia is added, to be certain that no substance insoluble in ammonia should remain in the liquid.

The ammonical solution is to be much diluted with water, deprived as much as possible of atmospheric air, and the oxide of nickel is to be precipitated by caustic potash. The oxides of cobalt and copper remaining in the solution, are deposited during the evaporation of the ammonia. The silica is to be looked for in the alkaline solution, by saturating it with muriatic acid, and evaporating it to dryness; water then dissolves the salts, and leaves the silica. Oxide of nickel, as well as cobalt, frequently contains silica, which must be separated from it by dissolving the oxide in muriatic acid, and by evaporation to dryness, which renders the silica insoluble. As to the separation of the metallic oxides with which those of nickel and cobalt may be mixed, I refer to what I have already stated.

The water in which the acid vapours are condensed, contains arsenic and sulphur; but if the mineral contains at the same time bismuth, zinc, antimony, or tin, the muriates of these metals will also be found in the liquid. This last circumstance would render the analysis extremely complicated, and I, therefore, do not describe it on the present occasion.

The bottle, H I, ought also to be furnished with a glass stopper. The interior of the tube, I K, is to be washed, the bottle is to be stopped, and left in a warm place, in order that the greater part of the sulphur precipitated may be acidified. If any portion of it remain, the bottle is to be unstopped and the liquid boiled; the sulphur agglutinates, and may be then conveniently washed, dried, and weighed.

In order to be certain that the acid liquor contains no iron in consequence of any mistake in the operation, nor any metals, the muriates of which are volatile, it is to be saturated as perfectly as possible with caustic potash. If any precipitate is formed, it is to be collected and examined. The solution is to be again rendered acid, and the sulphuric acid is to be precipitated by muriate of barytes. It is then proper to separate the excess of muriate of barytes by an addition of sulphuric acid; but this is not absolutely necessary. Afterwards a known quantity of iron dissolved in nitric acid is to be poured into the remaining liquid, and the oxide of iron and arsenic acid are to be precipitated by ammonia in excess. If the other ingredients have been determined, the quantity of iron required to precipitate the arsenic acid, may be estimated with more precision. For one atom of arsenic, two atoms of iron are to be used, which are to each other in weight, as three parts of iron to two parts of arsenic. Notwithstanding that the excess of oxide of iron increases the volume of the precipitate, it contributes to render the subarseniate less gelatinous, and more easy to edulcorate. The subarseniate ought to be twice heated, in order to be certain that it ceases to lose weight; for a small quantity of sulphuric acid often adheres very strongly to this precipitate.

As the arseniate of barytes is also an insoluble compound, I endeavoured to separate the arsenic acid from a neutral solution by muriate of barytes; but this method is attended with such inconveniences that it cannot be employed. In the first place, the arseniate of barytes which is precipitated is usually a mixture of neutral and subarseniate, and the liquid becomes acid, as happens with the phosphates of barytes, lime, &c. When then I wished to precipitate subarseniate of barytes, by adding excess of ammonia, I found that a considerable portion of subarseniate remained dissolved in the excess of alkali, as it is well known to happen with arseniate of lime. Lastly, I found that when arseniate of barytes is washed, the water never ceases to give a precipitate with sulphuric acid. I relate these circumstances in order to save others from fruitless labours.

ARTICLE XIII.

*On the Smelting of Tin Ores in Cornwall and Devonshire.**

By John Taylor, Esq. Treasurer of the Geological Society.

As I am not aware that the treatment of tin ores, or the mode of smelting them, has been recently described, and as the practice is confined to a certain district, it may be acceptable to the Society to have some account of the processes now used in Cornwall and Devon.

Tin ores are found in two kinds of deposits; first in veins accompanied by various other minerals; and, secondly, in alluvial matter in detached fragments.

It is usual in Cornwall not to apply the word ore to the oxide of tin, but to distinguish it, when in that state, by the term Black Tin, in contradistinction to white tin, which appellation is applied to it when smelted and in the metallic state.

The two kinds of tin ore above mentioned are, therefore, generally known by the names of Mine Tin and Stream Tin; and as they are for the most part smelted separately, and by different means, and as the metal produced from them is different as to its purity, it may be essential to point out the causes from which this diversity seems to arise.

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* From the Transactions of the Geological Society.

Mine Tin is, as I have mentioned, the produce of veins, and is raised with a mixture of all the substances which unusually accompany it. There are, not unfrequently, copper ores, pyrites, wolfram, micaceous iron, &c. and the separation of these, as also of the earthy matrix, is the object of various processes of dressing, which are conducted with the greatest care, and require a considerable portion of labour.

Whether, in a country where fuel for smelting is on the whole very cheap, it might not be economical to diminish the labour of dressing, and, by leaving more to be done in the furnace, reduce the expense of the former operations, is a question that I have never submitted to a direct experiment, though I conceive it to be one worthy of trial. The various earths may be quickly separated by fusion, as in the case of copper ores, which are now always smelted with a large mixture of the different kinds of spar in which they are found, all of which is easily run off by the fire, and the scoria or slag separated from the metallic part.

The fusibility of tin offers a mode by which it may be separated from an alloy of most other metals with which it is found to exist in veins, as lead and zinc ores are seldom mixed with it. This property is now made use of to a certain extent in refining tin, and might probably be taken advantage of still further, so as to avoid some of the charges incurred in dressing the ore.

The metal produced from Mine Tin is always of inferior quality, owing to the mixture of other metals, and which it is probable could not by any mode be entirely got rid of; it is known in commerce by the name of Common or Block Tin, and the quantity forms a large proportion of the whole that is brought to market.

Stream Tin is found in the lowest stratum of alluvial matter, in the bottoms of deep valleys, or places where a considerable deposit of mud, sand, and gravel, has been made by the action of water; it is often discovered occupying a thin bed incumbent on the rock, and covered by an *overburden*, as the streamers call it, which is sometimes from 20 to 70 feet thick. The tin is in rounded fragments, sometimes as large as walnuts, but more generally in the state of small gravel, and even of fine sand; it is imbedded in loose matter, composed of the detritus of the rocks from which it may be supposed to have been separated.

The principal peculiarity of Stream Tin is the absence of any other metallic mixtures, except nodules of hematitic iron ore, which sometimes accompany it. This circumstance fits it for producing a very pure metal. This is not the place to speculate on the causes which have so completely freed these ores from substances with which they were in all probability originally combined, or to inquire whether it is to be attributed to mechanical action, or whether it has been effected by decomposition; but it may be remarked that, besides the hematite already mentioned, only the indestructible metals, and the oxide of tin, are now discovered existing in deposits of this nature.

The operations of dressing Stream Tin are simpler than those for Mine Tin. It is smelted also in a different manner, and produces a superior metal known by the name of Grain Tin, which is principally used by the dyers, and for the finer purposes.

The processes for dressing Mine Tin are in many respects the same as are used for all other ores, but are subject to some variation, which are attributable to the following peculiarities.

1. Being for the most part found intimately dispersed throughout the matrix, the whole is necessarily pounded down to a very fine state to admit of the perfect separation of the ores.

2. That being unalterable by moderate degrees of heat, it admits of calcination, by which the specific gravity of the sulphurets or arseniats with which it is mixed, may be lessened, and a mode obtained of rendering them more separable.

3. That the weight of Tin Ore being greater than most others, it is less liable to waste in the processes of washing, and, therefore, may be dressed so as to be nearly clean from all substances not actually adhering to it.

From the first of these peculiarities it follows, that all tin mines must be furnished with stamping-mills of sufficient power to bruise down the ores raised, which is generally done so as to produce a minute division of the whole, and on this account, formerly, the quantity and fall of water that could be applied to this purpose usually limited the quantity of ore that could be returned from a mine, or the whole was frequently carried to some spot favourable to the erection of water-wheels to be applied to this purpose. Within a few years steam-power has been applied to stamping-mills, and has tended to increase the supply of tin ores. Engines for this purpose, of considerable power, are working with great effect at two of the largest tin mines in Cornwall, Wheal Vor and Great Huas; from which are now arising abundant returns of the metal, and where formerly it would have been impossible to have produced it.

The state of division, or the size, as the tin dressers call it, is regulated by a plate of iron pierced with small holes, through which the whole passes from the stamping-mill, being washed through by a rapid stream of water conducted upon it for the purpose. This is a point of great importance, and is regulated by the state of dissemination in which every ore is found.

It is not the intention of this memoir to detail the processes of dressing which are common to most ores, and, therefore, it may be sufficient to remark that, after being stamped, the tin ores are washed according to the usual mode, so as to separate the earthy mixture and as much of that of a metallic nature as is possible. All these operations are conducted with more than common care and accuracy; for as tin ore holds such a large proportion of valuable metal, it is of course treated with every precaution to guard against waste.

Some metallic substances will be found, however, which, from their specific gravity approaching nearly to that of tin ore, or rather exceeding it, cannot be removed by any process of washing; these are mostly decomposable by a red heat, which the oxide of tin will bear without alteration. Therefore, after as much has been done as possible to render the ores clean on the dressing-floors, they are taken to the *burning-house*, which is furnished with small reverberatory furnaces, on the floor of which the ores are spread and submitted to the action of a moderate and regular fire: they are frequently turned over by an iron rake to expose fresh surfaces, and a considerable volatilization of sulphur and arsenic takes place; the former seems principally to be consumed, and the latter is condensed by long horizontal flues constructed for this purpose. After the ores come from the burning-house, the process of dressing is completed by further washing, which is rendered easy by the alteration which has been produced in the relative weight of the substances.

Copper ore is not unfrequently present in these cases, and, as it is in part converted into sulphate of copper, the water which is first used is preserved, and a portion of copper obtained from it by means of iron.

The great specific gravity of the tin ore, as I have before remarked, renders it possible with care to subject it to many operations in dressing without much waste; and they are, therefore, applied until the whole is generally so clean, as to yield a produce of metal equal to from 50 to 75 per cent. In this state they are sold by the miner to the smelter, who determines their value by assaying a sample, carefully taken from the whole quantity.

The furnaces for smelting Mine Tin are all of the common reverberating kind, and are of sufficient size to hold twelve to sixteen hundred weight of ore.

The charge is prepared by mixing it with a proportion of stone coal, or Welch culm, to which is added a moderate quantity of slaked lime; these are turned over together and moistened with water, which prevents the too rapid action of the heated furnace, and which would otherwise volatilize some of the metal before fusion commenced.

The heat employed is a very strong one, and such as to bring the whole into perfect fusion; it is continued seven or eight hours, when the charge is ready to draw. For this purpose, the furnace is furnished with a tap-hole leading from the lowest part of the bottom, which, during the process, is stopped with clay or mortar, and under which is placed an iron kettle to receive the metal. The furnace has also a door at the end opposite the fireplace, through which the slag or scoria may be raked out from the surface, while the tin is flowing out by unstopping the tap-hole.

They are thus divided, and the tin is laded into moulds, so as to form plates of a moderate size, and put by for a further refining. The slag, which rapidly hardens into a mass, is removed to a dressing-floor, where, being broken up and stamped, it is washed, and a quantity of tin taken from it, which is called *Prillion*, and which is afterwards smelted again.

No operation in smelting is more easy than that practised for tin ores, nor is there any one in which the reasons for the mode of treatment are so obvious. There are but two things to accomplish in this first process; to obtain perfect fusion of the earths so as to suffer the metal to separate easily from them, and to decompose the oxide of which the ore uniformly consists.

The addition of lime contributes to effect the former, and that of carbonaceous matter or coal completes the reduction of the ore. The separation of the metal from the earths then takes place in the usual way during fusion, by the difference in their specific gravities, the one precipitating to the bottom of the furnace, from whence it is drawn off by the tap-hole, and the other, floating on the surface, is removed in the manner I have described.

The plates of tin, which are the produce of this smelting, are somewhat impure, and are more or less so according to the quality of the ore which has been used; they are reserved until a sufficient quantity of them are obtained to proceed with the refining, which is performed either in the same furnace, after ore-smelting is finished, or in a similar one, which may be reserved for the purpose.

All the processes for refining metals in the fire must be performed by taking advantage of some property in which the metal operated on may differ from those with which it is alloyed, and which it is intended to separate from it. These differences may consist in the facility or difficulty of oxidation, in their tendency to volatilize, in the temperature required for fusion, or in their relative specific gravities.

Upon an attention to the two latter circumstances are founded the operation for refining tin. The substances which are most to be suspected in the produce of the first melting, and which it is desirable to separate, will probably be iron, copper, arsenic, tungsten from the wolfram, which the miners call mock-lead, and a portion of undecomposed oxides, sulphurets, or arseniates, and of some earthy matter or slag.

The furnace for refining is raised but to a very moderate degree of heat, and the plates of tin being placed in it are suffered to melt very gradually, and the metal flows from the furnace at once into the kettle, which is now kept hot by a small fire placed beneath it. The more infusible substances will now be left in the furnace, and a further purification of the tin is obtained by agitating it in the kettle for some time by an operation which they call *tossing:* this is performed by a man with a ladle, who continues for some time to take up some of the melted metal, and pour it back into the kettle from such a height as to stir up the whole mass and put every part into motion.

When this is discontinued, the surface is carefully skimmed, and the impurities thrown up are removed; these consist of such matters as are lighter than the tin, but which are suspended in it, and, being disengaged by the motion, find their way to the top. In general, the metal is at once laded into the moulds, after the tossing and skimming is completed, but the produce of impure and irony ores may yet require that the tin be divided as much as possible from the mixture which may yet remain. This may be effected in a great degree by keeping the mass in the kettle in a melted state, by which the parts which are heavier than the tin will sink to the bottom, and by leaving a proper portion behind, the tin will be materially improved.

The last operation is that of pouring the metal into moulds, which are usually formed of granite, and which are of such a size as to make it into pieces of somewhat more than three hundred weight each. These are called blocks, and are sent according to the provisions of the Stannary laws, to be coined by the Duchy Officers, and it then comes to market under the name of Block Tin, or a certain part which has been treated with more than common care, is called Refined Tin.

The making of Grain Tin from the ores from stream works is conducted in a manner altogether different, and remains to be described.

I have pointed out the purity of these ores, as regards their freedom from a mixture of other metals, and I do not think it important here to describe the mode of separating them by washing from the sand and gravel in which they are found, because the processes are very similar to those in use for dressing other ores. The stream tin is generally made very clean, and is carried in this state to be sold for smelting, to establishments which are called Blowing-Houses, being thus distinguished from SmeltingHouses in which Mine Tin is reduced, and the term is also descriptive of the process employed.

The reduction of the ores for Grain Tin is performed by blast furnaces, and the only fuel used is charcoal. This mode of smelting is exceedingly simple, and is probably the most ancient one, as would appear from relics sometimes met with of furnaces of rude construction, and in some of which the wind alone seems to have been depended on for urging the fire.

The furnaces now in use are similar to those met with for smelting iron in foundries where the blast is used, and are formed by a cylinder of iron standing upon one end and lined with clay or loam. The upper end is open for receiving the fuel and ore which are thrown alternately, and a hole at some distance from the bottom at the back of the cylinder is provided to admit the blast, and another, lower down and opposite to it, suffers the metal to flow out regularly as it is reduced.

A strong blast is kept up by bellows, or, in more improved works, by pistons working in cylinders, and the air is conducted by a proper pipe so as to blow into the orifice in the furnace.

The only purification it seems to require is to separate from it such substances as are mechanically suspended in it, and for this purpose it is laded into an iron pan or kettle where the fusion is kept up by a gentle fire underneath, and a complete agitation of the mass is effected by plunging into the melted metal pieces of charcoal, which have been soaked in water, and by means of an iron tool, keeping them at the bottom of the kettle. The water in the charcoal is rapidly converted into vapour, which rushing through the metal, gives it the appearance of rapid ebullition. After this is over, and the whole has rested some little time, the scum, which is thrown up to the surface, is taken off, and the tin, which is peculiarly brilliant in appearance, is removed by ladles into proper moulds to form the blocks in which it is generally sold.

Grain Tin is, however, sometimes put into a different form by breaking it: for this purpose the blocks are heated to such a degree as is known to render the metal brittle; they are then raised a considerable height from the ground, and, being suffered to fall, the whole divides into fragments, which assume a very peculiar appearance.

The smelting by a strong blast is injurious to metals that are volatilizable by heat, as they have in this mode no protection from the slag, which in reverberating furnaces floats on their surface, and protects them from oxidation and evaporation. The old practice of melting lead in what are called Ore Earths, is, on this account, giving way, and reverberating furnaces are coming into general use, by which the produce of metal from the ore is considerably increased. Tin, though volatile to a certain degree, is not affected by the process in any important manner, but as some flies off in white fumes, it is usual to construct a long horizontal flue, which is made to communicate with and pass through a kind of chamber, in which a considerable part of these fumes is condensed and collected.

ARTICLE XIV.

*Analysis of Two Varieties of Native Carbonate of Manganese.**
By M. Berthier.

THE existence of carbonate of manganese has been long since stated by several chemists; but as some mineralogists still entertain doubts on the subject, I think it may be useful to publish the analyses which I have performed of two minerals that are essentially composed of carbonate of manganese. One of them is from Nagyac, and was sent to me by M. Cordier; and the other, from Freyberg, was brought by M. de Rivero.

The carbonate of manganese from Nagyac accompanies the ores of gold, tellurium, &c.; it is very much mixed with lamellar quartz; it is of a flesh-red colour, and transparent at the edges; its powder is white; it becomes brown by calcination; it dissolves in cold nitric acid, with the evolution of carbonic acid gas. The solution gives a yellow precipitate with the hydrosulphurets, which shows that no iron is present; it does not contain the smallest trace of magnesia.

A portion of this mineral was dissolved in nitric acid, 0·21 of quartz remained unacted upon. The manganese was separated from solution by an hydrosulphuret, and the lime was afterwards precipitated by an oxalate, the calcined precipitate gave 0·043 of lime.

Another portion was treated with pure sulphuric acid, and the residuum was well dried to expel the excess of acid; this residuum weighed 1·245; deducting 0·21 of quartz and 0·103 of sulphate of lime which it must contain, there remain 0·932 of of sulphate of manganese, equivalent to 0·443 of the protoxide of this metal. According to these experiments, and determining the quantity of carbonic acid by the deficiency, this mineral is composed of

Quartz	0·210
Protoxide of manganese	0·443
Lime	0·043
Carbonic acid	0·304
	1·000

Which, deducting the quartz, gives:

Protoxide of manganese	0·560
Lime	0·054
Carbonic acid	0·386
	1·000

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* From the Annales des Mines.

Or,

Carbonate of manganese	0·905
Carbonate of lime	0·095
	1·000

This result may be considered as correct, because, supposing pure carbonate of manganese to contain one proportion of carbonic acid, it does not differ more than 0·005 from that which it really contains.

The carbonate of manganese from Freyberg occurs abundantly in a copper and lead mine. It is amorphous, lamellar, and the laminæ are slightly curved. Its colour is flesh-red, translucid, brittle, easily scratched and powdered.

Besides oxide of manganese, it contains some oxide of iron, lime, and magnesia. These four substances were separated by the usual means, and the quantities of iron, lime, and magnesia, carefully ascertained.

To determine the quantities of manganese and carbonic acid, a portion of the mineral was exposed to heat and air, in order to peroxidize the metals, and afterwards it was strongly heated to expel all the carbonic acid, and to convert the manganese to the state of red oxide; the residuum weighed 0·655, the loss being consequently 0·345. By deducting from the weight of the residuum the sum of the weights of the lime, magnesia, and peroxide of iron, the weight of the red oxide of manganese is ascertained, from which that of the protoxide is deduced.

On the other hand, by adding to the loss of weight by calcination, the weight of the oxygen absorbed by the protoxides of iron and manganese, the proportion of carbonic acid is determined. The results were:

Protoxide of manganese	0·510
Protoxide of iron	0·045
Lime	0·050
Magnesia	0·008
Carbonic acid	0·387
	1·000

Or,

Carbonate of manganese	0·822
Carbonate of iron	0·073
Carbonate of lime	0·089
Carbonate of magnesia	0·016
	1·000

This analysis agrees perfectly with the theoretical composition of the carbonates of manganese, iron, lime, and magnesia.

It is quite evident that these four carbonates are merely mixed, but intimately so, in the minerals from Nagyac and Freyberg.

Article XV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

April 25.—On the Mechanism of the Spine, by Mr. Earle.

Observations on the Eclipse of August, 1821, by Mr. Dawes.

May 2.—On the Nerves which associate the Muscles of the Chest in the Actions of Breathing, Speaking, and Expression, by Charles Bell, Esq.

A short Account of some Appearances in the Moon on the 24th of April, by Mr. Lawson.

May 9.—Experiments and Observations on the Newry Pitchstone, and on the artificial Formation of Pumice, by the Right Hon. J. Knox.

May 16.—On the Changes which the Egg undergoes during Incubation, by Sir E. Home, Bart.

May 23.—On the Mathematical Laws of Electro-magnetism, by P. Barlow, Esq.

On the Heights of Places in the Trigonometrical Survey, by B. Bevan, Esq.

GEOLOGICAL SOCIETY.

Feb. 15.—A brief notice was read, accompanying specimens of rocks from Bermuda, by Captain Vetch, MGS. &c.

Mr. Joseph Wood's paper, on the Rocks of Attica, was read.

March 1.—An essay on the Geology of Nice, by M. A. Risso, was read.

Nice, the capital of the Maritime Alps, is placed at the foot of an almost insulated rock on the shore of the Mediterranean. The tract around the city, which is described in the present paper, is bounded on the west by the river Var, and on the north and east is protected by some of the last ranges of the Alps, and by the calcareous summits on the shore of the Mediterranean. The rocks within this tract are principally composed of limestone; but on the west and north-west of Nice, the surface of a large district consists of clay abounding in siliceous pebbles. The author describes these rocks in detail, and states their local situation and boundaries.

The calcareous tract is composed of three principal varieties of rock: 1. Fine-grained compact limestone, distinguished in the country by the name of Paglione, which is of a bluish-grey colour, and becomes yellowish, and falls to pieces by exposure. It has in some places a granulated appearance, is of difficult solution in nitric acid, and is not convertible into lime by calcination; it contains the remains of a number of marine organized bodies at present unknown in the Mediterranean Sea. 2. The "Calcaire Subalpine" of Brongniart. This rock is generally greyish-white, of various shades; it is almost entirely soluble in nitric acid, and affords an excellent lime. Near the city, it is stratified; the beds are inclined at an angle of about 40° with the horizon, and contain vast rents, crevices, and grottos. It abounds in petrifactions, which are enumerated by the author. 3. A third kind of limestone, incumbent upon those first mentioned, is of a grey colour, almost even fracture, and of considerable specific gravity and hardness. It exhales, when breathed upon, an earthy smell; is partly soluble in nitric acid, and by calcination forms a very strong lime. The author considers it as nearly the same with the calp of Ireland, described by Kirwan, and analyzed by the Hon. Mr. Knox.

Clay marl, with chlorite (marne chloritée) is placed above the limestones, and from the variety of its characters, and of the containing fossils, appears to have been formed at different epochs: the most ancient is of an olive-green colour; is mixed with grey limestone and chlorite; and is distinguished by the great abundance of its fossils, which are altogether different from those contained in the other varieties of marl.

The marl (argile calcifère), which succeeds the last mentioned variety, is considered by the author as similar to that which extends from Piedmont to the Appennines; and from thence, without interruption, to Abruzzo and Puglio; and which in the Maritime Alps, obviously lies over the limestone, and descends from north to south, to form the chain of hills extending from Montcao to the sea. It contains shells in great abundance and variety, some having little or no resemblance to species at present known; while the types of others are found on the adjoining sea. Many of the more recent species resemble those of Grignon, and appear to have been deposited at the same era. The author describes several depots of such shells in the vicinity of Nice, and enumerates the species which they respectively contain, amounting to more than 200.

The pebbles (galets) mixed with, or incumbent on, the marl beds, form an extensive deposit, in layers, which generally range from north to south, and are inclined at a small angle with the horizon. The pebbles are composed of several kinds of limestone and sandstones, with petrifactions, quartz, greywacke, and various primitive rocks. Another class of substances, or compounds, comparatively recent, but still of prior formation to the latest catastrophe produced by the sea, consists of marble, breccia, puddingstone, sand, and clay.

The marble, to which the title of Mediterranean has been given, from the great number of Mediterranean shells which it contains, is a very hard and compact calcareous breccia, either white or coloured. It contains the remains of various molluscæ and zoophytes; the shells being in most cases squeezed together, but in other respects in perfect preservation.

The puddingstone consists of clay and sand, cementing rounded gravel and the remains of shells of existing species, almost all broken down, like those on the sea shore after stormy weather, and mixed with bones of quadrupeds and fish.

Of breccia, several varieties are found in the vicinity of Nice. The most ancient somewhat resembles the nagelflue of Switzerland, and is found above the Alpine and Subalpine limestones. The most recent breccia has a cement of the Mediterranean limestone, or of reddish clay, and sometimes contains shells and fragments of the bones of various quadrupeds and of birds. A third variety of breccia contains only the remains of land shells; and a fourth, resembling that of Gibraltar, fills a cavern in the compact limestone, and contains the remains of bones, teeth, and horns, much broken down, and so much decomposed as to retain their form and cohesion only by means of the cement which unites them.

A very extensive deposition of whitish sea sand is found on the south side of the Bay of Villa-Franca beneath a reddish soil of several metres in thickness; and the author enumerates nearly 200 species of shells collected at this place. A deposition still more recent consists of argillo-calcareous earth of various shades of red, grey, and white; and immediately above it is the vegetable soil.

The author infers from the facts now stated, that the sea has been the sole agent in producing the various appearances and combinations of mineral substances, which he has described; and he concludes by stating his opinions as to the nature and progress of the marine agency which has produced or modified the deposition of the several rocks, and of the fossil remains which they contain.

March 15.—A notice on the Rocks of Attica, by Joseph Woods, Esq. MGS. was concluded.

Attica is a promontory bounded on two sides by the sea, and divided from the remainder of the Grecian continent by a range of mountains, the highest point of which, the ancient Parnes, may be about 4000 feet above the sea.

Within the triangular space thus defined are also numerous mountains very irregularly disposed. The basis of all the country appears to consist of primary rocks, principally of mica slate, with granular limestone of several varieties; these constitute the greater part of many of the mountains, and appear in the plains wherever the rock is exposed to a sufficient depth.

Above the primary rocks is a conglomerate, consisting of primary substances, imbedded in calcareous paste which contains magnesia.

A series of calcareous rocks, including a compact limestone of a splintery fracture, of various shades of grey and buff, forms the mass and superior parts of the range of hills which divides the plain of Athens.

The hills of the Piræus and Munychia are composed of a soft calcareous stone containing magnesia, and including organic remains.

April 19.—A letter was read from Sir Alexander Crichton, accompanying a specimen of fossil shells from the neighbourhood of Tunbridge Wells.

The basis of the country around Tunbridge Wells is well known to be composed of ferruginous sandstone, and it would appear that the remains of organized bodies are very rarely found in it. The specimen presented by Sir Alexander Crichton occurred in a quarry on the side of the Groombridge road, adjoining the property of Mr. Powell. The petrifactions which they contain are described as "occupying small cavities in the sandstone rock, which are filled with ovate-shaped masses of ironstone, apparently composed of sand and clay, and the casts of shells."

The blocks of stone split easily, and on the surface thus disclosed exhibit innumerable fine casts of shells, but in no instance have any remains of the shells themselves been found. The author states various considerations to account for the appearances and situation of these remains.

Sir Alexander Crichton subjoins a statement that in sinking a well recently at Tunbridge Wells, coal was met with at the depth of 50 or 60 feet from the surface; the larger, however, was so thin that it was not expected to be useful.

A letter was read from the Rev. John Rogers, of Exeter, containing a sketch of the Geology of Haldon Hill.

The road from Exeter towards Elphinstone, for the first mile and a half, consists of alluvial soil, containing silicious and argillaceous pebbles; to this, red marl succeeds, which is continued to within a quarter of a mile of the summit of Haldon Hill. The beds dipping NE. and NW. at angles of 5° to 10° with the horizon. The construction of the new road between Exeter and Chudleigh has recently afforded a very distinct section of some of the rocks, of which Haldon Hill near its summit is composed.

Ascending the hill, the road is cut chiefly through the red marl, which, near the top, contains pieces of rolled granite, and of claystone porphyry of several varieties. These, with other substances, form a sort of bed, from six to twelve inches thick, in which the porphyry predominates; and about a quarter of a mile from the highest point of the hill, the red marl is succeeded by a bed of yellow sand; the junction being abrupt, without waving or intermixture. Above this sand, on every part of Haldon examined by the writer, a bed of flints was found.

Above the junction, the sand is traversed by an irregular bed of yellowish-grey sandstone, which, in some places, assumes a vitreous quartzose aspect, and an olive-green colour, approaching that of some varieties of pitchstone; the sand contains fragments of shells and corallines in considerable quantity.

We defer a further account of the proceedings of this Society till the appearance of a new Part of their Transactions, which is now in the press, and will be published within a few weeks. This part will contain, in addition to the papers of which an account has been already given in this journal, a memoir, "On the Excavation of Valleys by diluvian Action, as illustrated by a Succession of Valleys which intersect the South Coast of Dorset and Devon," with a Map and Views, by the Rev. W. Buckland, Professor of Geology in the University of Oxford, &c. &c.; and "Additional Notices on the Fossil Genera Icthyosaurus and Plesiosaurus," with several Plates, by the Rev. W. Conybeare, MGS.

We have great pleasure in informing our readers that the price of the Geological Transactions will in future be considerably reduced; the Society having recently taken upon itself the expense and risk of the publication, and consulted economy by the adoption of a fuller page, and the substitution of lithographic plates for engravings on copper.

ARTICLE XVI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

1. *Erratum in Dr. Thomson's Paper "On certain Saline Solutions which may be cooled, &c."*

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR,

In the printing of a paper of mine "On certain Saline Solutions which may be cooled, &c." inserted in the *Annals of Philosophy*, vol. iii. p. 169, a mistake has been committed of such magnitude as to render the paper nearly unintelligible. On that account, I think it necessary to point it out to the attention of the reader. The error is this:—A pretty long paragraph which ought to have followed the word *water*, at the bottom of p. 170, has, by some strange mistake, been inserted at the end of the paper. The portion of the paper at present at the end, and beginning with, "It will appear from what follows," in line 25, p. 174, should be placed at the bottom of p. 170.

I am, dear Sir, yours truly,

THOMAS THOMSON.

II. *Composition of Formic Acid.*

Dr. Gobel saturated distilled formic acid with oxide of lead, and found that the formate of lead contained 24·5 per cent. of formic acid. In heating 10 grains of formate of lead with black oxide of copper in a glass tube, he obtained 5·4 cubic inches of carbonic acid gas, which, according to the state of barometer and thermometer, were equal to 2·922 grains of carbonic acid = 0·792 grains of carbon. The glass tube weighed after the experiment 3·55 grains less than before, which would make the water amount to 0·68 grain = 0·0755 grains of hydrogen. The composition of 2·45 grains of formic acid will, therefore, be:

Carbon	0·797
Hydrogen	0·075
Oxygen	1·578
	2·450

The formate of lead is composed of

1 atom oxide of lead	107·5
1 atom formic acid	34·9
1 atom water	8·45
	150·85

One atom of formic acid = 34·9 must, therefore, be composed of

Carbon	11·35
Hydrogen	1·06
Oxygen	22·43
	34·84

Which is equivalent to

2 atoms of oxide of carbon	26·4
1 atom water	8·45
	34·85

According to Dr. Thomson's numbers, formate of lead would be composed of

1 atom oxide of lead	112
1 atom formic acid	37
1 atom water	9
	158

and the formic acid composed of

2 atoms of oxide of carbon	28
1 atom of water	9
	37

III. *Effects of Boracic Acid on the Acidulous Fluates of Potash, &c.*

Dr. Zeise being engaged in some experiments on the fluoboric acid, and on the fluoborates of the alkalies, observed that a solution of fluate of potash in which the acid was in excess, might be rendered alkaline by the addition of boracic acid. He dissolved carbonate of potash in fluoric acid, and used so much of the acid that the solution reddened litmus paper very sensibly, and the free acid did not disappear even when the solution had been kept boiling for a quarter of an hour. When boracic acid that had been melted, dissolved in water and crystallized again, was added, it easily dissolved. Now a solution of litmus showed much less acid than before. After the addition of a new quantity of boracic acid, no trace of free acid could be found by litmus; and after a fresh addition of boracic acid, the solution exhibited the appearances of alkali; for a solution of litmus reddened by the above-mentioned fluate of potash became blue when mixed with a few drops of this solution. When the quantity of boracic acid was further increased, the solution became again acid. Not a trace of acid was lost during this experiment under the form of vapour or gas; for moistened litmus paper placed above the liquid had not in the least changed its colour. Pure fluoric acid and pure boracic acid, when mixed in different proportions, never showed any similar appearance, nor was it expected.

A solution of litmus reddened by a solution of boracic acid was mixed with another solution of litmus reddened by acidulous fluate of potash, and a blue colour instantly appeared. The same effect took place when ammonia or soda was substituted for potash. Litmus paper, when reddened by the bifluate of potash, became red when put into a solution of boracic acid, either in alcohol or in water. Litmus paper reddened by boracic acid was turned blue by the acidulous fluate of potash, and the blue colour was again changed to red by another acid.

Syrup of violets reddened by the fluate of potash became, on addition of boracic acid, first blue, afterwards green. Turmeric paper and logwood paper suffered corresponding changes of colour, so that all tests proved the disengagement of the alkali by the addition of boracic acid.

This very curious observation of Dr. Zeise seems clearly to prove that the fluoboric acid neutralizes less alkali than any one of its constituent parts would saturate alone. Analogous, but not so remarkable, is the hyposulphuric acid which neutralizes only as much of a base, as each of its component acids neutralizes separately.

[Many of the facts here stated may be explained by Mr. Faraday's discovery, that boracic acid reddens turmeric paper.—*Edit.*]

(See Institution Journal, vol. 6, p. 152; and vol. 11, p. 403.)

IV. *Analysis of Lepidolite.* By Dr. C. G. Gmelin and Winz.

Gmelin some time since observed, that the lepidolite from Utoën, when treated with carbonate of barytes, had acted upon the platinum of the crucible, he therefore suspected it to contain lithia, and found it in the following way: He boiled finely powdered lepidolite with sulphuric acid to dryness, and dissolved the residuum in water. By slow evaporation, fine crystals of alum were obtained. The remaining solution was saturated with carbonate of ammonia, and after the removal of the precipitate, evaporated, and the salt heated to redness. The sulphate was dissolved, and after having deposited some traces of manganese, it was mixed with hydrosulphuret of ammonia, the excess of which was removed by heating the liquid. The sulphate was then decomposed by acetate of barytes, the acetate evaporated, and by heat converted into carbonate. All the lithia and potash were dissolved by repeated boiling in water, the solutions evaporated to dryness, and the residuum was repeatedly washed with cold water to remove the potash, so that lithia only remained.

Dr. Gmelin found that muriate of lithia gives a beautiful purple colour to the flame of burning alcohol. The sulphate does the same when first dissolved in water, and then thrown down by pure alcohol; the alcohol burns with a purple flame, and of course no strontian could be present in this experiment. The flame with lithia from lepidolite from Rozena, in Moravia, had green edges like that of the borates. Lithia of the lepidolite from Utoën exhibited this phenomenon much less distinctly. . . . .

V. *Analysis of the Red Lepidolite from Moravia.*

It had been ascertained previously that this mineral, besides lithia and potash, contained silica, alumina, oxide of manganese, and fluoric acid.

In order to determine the quantity of fluoric acid, 30 parts of lepidolite were mixed with 100 parts of dry carbonate of soda, and kept red-hot during an hour. The fused mass was repeatedly boiled with water, until the water showed no trace of an alkali. Carbonate of ammonia was now mixed with the solution, and while slowly evaporating, a small quantity of the same salt was occasionally added. Silica and alumina fell down, and were separated by a filter. The liquid, after having been saturated with muriatic acid, was kept warm for some time to expel the carbonic acid, which the water might have dissolved, and then mixed with pure ammonia and muriate of lime in a well-stopped vial. A white bulky precipitate appeared; it was washed with the same precautions against the influence of carbonic acid, and, when dried, weighed 3·77 parts. Sulphuric acid occasioned the appearance of copious vapours, that had the smell of fluoric acid, and which corroded glass.

Fluoric and phosphoric acid being so frequently associated in nature, some experiments were made to detect the latter acid. By exposure to heat, the fluoric and free sulphuric acid were expelled, and when the alcohol was poured on the remaining sulphate of lime, and afterward evaporated, there remained a substance which, after fusion, had a glass-like appearance, which deliquesced when exposed to the air, and was dissolved with the greatest facility in pure water: in this solution, limewater caused a white precipitate. It was, therefore, phosphoric acid, and the phosphate of lime weighed 0·06 part, which, deducted from the above quantity, leaves 3·71 of fluate of lime; accordgin to Berzelius, equal to 0·1033 of fluoric acid, or 3·44 per cent. The phosphate of lime contained 0·00337 of phosphoric acid, or 0·112 per cent.

Thirty parts of lepidolite, fused in the usual way with potash, dissolved in muriatic acid, evaporated to dryness, and washed, gave 14·717 silica, or 49·06 per cent. The liquid that held the soluble parts in solution was mixed with excess of pure potash, and almost the whole of the precipitate, which appeared at first, was redissolved. The alkaline solution, when neutralized with muriatic acid, and precipitated by carbonate of ammonia, gave 10·08 alumina = 33·61 per cent; and the remaining liquid not giving any precipitate by evaporation, there could have been no glucina in the mineral. The powder which remained undissolved by the potash consisted of 0·408 per cent. magnesia, and 1·402 per cent. oxide of manganese, with a trace of oxide of iron.

These two analyses give, therefore, the following results:

Silica	49·06
Alumina	33·61
Magnesia	0·408
Oxide of manganese	1·402
Oxide of iron	Trace
Fluoric acid	3·44
Phosphoric acid	0·122
	88·042

Experiments in order to ascertain the quantity of the alkaline bodies.

Sixty parts of lepidolite, mixed with 240 of carbonate of barytes, were kept red-hot for two hours. The mass was dissolved in muriatic acid, the barytes thrown down by sulphuric acid, and at last sufficient carbonate of ammonia was added to precipitate all the earths and metallic oxides which had been dissolved. When the solution was afterwards evaporated, and the salt heated, there remained 12·7 of a sulphate. It still contained some oxide of manganese, and to remove that, it was dissolved in water, and then mixed with hydrosulphuret of ammonia. Now the alkaline sulphate still weighed 12·36 parts, and by muriate of platinum, there was obtained 3·5165 potash = 6·503 sulphate of potash, so that there remained for the sulphate of lithia 5·857 parts. However, after having removed all platinum from the solution, there remained 7·7 parts of pure sulphate of lithia, which Dr. Gmelin considers as the correct quantity, it being difficult to free the triple muriate of potash and platinum by washing, from all muriate of platinum. It is, therefore, probable, that the quantity of sulphate of potash is too great, and that it would only be 0·466 = 0·251965 potash, or 4·1866 per cent.

Supposing 100 parts of sulphate of lithia to contain 27·99 lithia, the quantity of lithia would amount to 3·592 per cent.: 100 parts lepidolite lost in the fusion, in a violent heat, 1·947 per cent. which, besides water, certainly consisted of fluoric acid and silica. The composition of lepidolite is, therefore,

Silica	49·06
Alumina	33·61
Magnesia	0·408
Oxide of manganese	1·402
Oxide of iron	Trace
Fluoric acid	3·44
Phosphoric acid	0·112
Potash	4·186
Lithia	3·592
Water and loss	4·190
	100·000

An analysis was made expressly for the purpose of ascertaining whether lime existed in the mineral, and in that case, the fluoric acid, perhaps, might be combined with it, as Vauquelin supposes; but not a trace of lime was found.

In a comparative analysis of the lepidolite from Utoën, there was found 1·482 per cent. of fluoric acid, and 0·7508 of phosphoric acid, and the mineral appeared to be mixed with quartz, upon which the greater hardness depends.

Ros had discovered fluoric acid in the white mica, and MM. Gmelin and Winz found, in mica from Braddbo, near Fahlun, in Sweden, 1·9512 per cent. fluoric acid, but no phosphoric acid.

Sulphate of lithia from the lepidolite, according to an analysis by acetate of barytes, consisted of

	Per cent.
Sulphuric acid	72·52
Lithia	27·48
	100·00

According to an analysis by means of acetate of lead:

	Per cent.
Sulphuric acid	71·60
Lithia	28·40
	100·00

It appeared, therefore, highly probable, that Arfwedson and Gmelin had overlooked another substance in the alkali of the petalite. In repeating the analysis of petalite, Dr. Gmelin obtained soda.

Arfwedson found sulphate of lithia composed of

Sulphuric acid	68·65
Lithia	31·35

VI. *Dr. Gmelin on the Tourmaline from Karingsbrakka, in Sweden.*

When Dr. Gmelin was at Stockholm in 1816, he analyzed the tourmaline from Karingsbrakka, but had a loss of more than 10 per cent. At that time he could not find boracic acid, though he had directed his attention to it. The great loss made him afterwards repeat his analysis; but still he had a considerable deficiency, though not so great as formerly. He heated the powdered tourmaline in the usual way with carbonate of barytes, dissolved in muriatic acid, and obtained silica in the ordinary mode.

He threw down all barytes from the solution by sulphuric acid, neutralized the excess of acid afterwards with pure ammonia, and precipitated the iron and alumina by carbonate of ammonia, and separated these two substances from each other by potash.

The magnesia remained as a triple salt in the solution, which now was evaporated to dryness, and heated red-hot, dissolved again in water, when a small quantity of silica remained. The liquid was then decomposed by acetate of barytes, filtered, evaporated, and the salt heated. The dry mass was washed with water to dissolve the alkaline bodies, and there remained carbonate of barytes and magnesia, which again were dissolved by muriatic acid, mixed with sulphuric acid, filtered, evaporated, and heated, and the quantity of magnesia ascertained.

The alkaline solution, after having been neutralized with sulphuric acid, was evaporated; during the evaporation, some crystals of boracic acid appeared which were easily dissolved by alcohol.

The solution of the alkaline sulphate showed by its crystallization that it contained potash and soda; the presence of the latter was also clearly shown when the sulphate was converted into a carbonate, and neutralized with bitartrate of potash. Beautiful crystals of tripletartrate of potash and soda appeared. Dr. Gmelin recommends this method to ascertain the presence of soda in the alkalies from minerals.

This tourmaline was found to be composed of

Silica	38·92
Boracic acid	0·60
Alumina	33·24
Oxide of iron	7·20
Magnesia	9·80
Potash and soda	2·53
Loss by heat	0·03
	92·32

A direct analysis by sulphuric acid gave:

Silica	42·59
Boracic acid	0·60
Alumina	34·32
Oxide of iron	5·22
Magnesia	8·47
Potash and soda	2·42
	93·62

The quantity of boracic acid was not ascertained in the latter analysis, and the greater quantity of silica may be easily explained, partly by a small quantity of undecomposed mineral, and partly by the decomposition of a minute portion of glass.

To explain the unusually great loss, Dr. Gmelin tried the tourmaline with oxide of copper, but no trace of carbonic acid appeared. He thought it possible that some earth in the tourmaline might in this combination contain more oxygen than usual; therefore, he boiled the powder with concentrated sulphuric acid, but no air, except that of the vessels, appeared. It remains, therefore, still dubious what this substance is which escapes during the analysis, but it may be partly boracic acid, all methods for determining its quantity in minerals being deficient. Dr. Gmelin proposes as the best method, to heat tourmaline with carbonate of soda, dissolve in water, to precipitate all the earths by carbonate of ammonia, to saturate the solution perfectly by nitric acid, and then to precipitate the boracic acid with nitrate of lead.

VII. *Lampic Acid.*

In the 6th vol. of Institution Journal, Mr. Daniell published an account of the acid formed by the slow combustion of ether, and which, for reasons that are well known, he denominated lampic acid.

The circumstances under which this acid is generated, connected with the fact that a given weight of it combined with barytes, yielded almost precisely the same quantity of sulphate of barytes, as would be given by an equal weight of acetate of barytes, induced me to suspect that the acid in question was not a peculiar, but merely acetic acid; the difference depending upon an admixture of ether. Mr. Daniell has since repeated and published the results of his experiments.—(Institution Journal, vol. 12, p. 64.) Several of these I had an opportunity of witnessing, and was certainly persuaded that my first impression was erroneous, and that the compound possessed such properties, as entitled it to be considered as a peculiar acid. By continuing and varying his experiments, Mr. Daniell has, however, arrived at the conclusion, that the acid formed during the combustion of the ether is, in fact, the acetic; but combined with a substance of a highly disoxygenizing nature, different from ether, and of a resinous quality, and which Mr. Daniell considers to be a compound of hydrogen, carbon, and azote, and he has named it hydro carburet of azote. It appears to consist nearly of

4 atoms of carbon	30·0
1 atom of azote	17·5
11 atoms of hydrogen	14·5

(*Edit.*)

VIII. *Preservation of Anatomical Specimens.*

Dr. Macartney, of the Dublin University, has for some time employed a solution of alum and nitre, for the purpose of preserving anatomical preparations. He finds that it preserves the natural appearances of most parts of the body, more completely than spirits, or any other fluid heretofore used. The proportions of the alum and nitre, and the strength of the solution require to be varied according to circumstances; and in order thoroughly to impregnate the anatomical preparation, the liquor must be for some time occasionally renewed. The solution possesses such antiseptic powers, that the most putrid and offensive animal substances are rendered perfectly free from fœtor by it in a few days.—(Med. Rep. xvii. p. 169.)

IX. *Native Nitrate of Soda.*

M. Mariano de Rivero states that a bed of nitrate of soda, several feet thick, and more than 40 leagues in length, has been discovered in Tarapaca, a district of Peru. In some places, the bed appears at the surface; it is sometimes in a crystalline state, but most frequently mixed with clay and sand; it is deliquescent, and suffers the same changes in the fire as nitrate of potash. The place where it occurs is three days' journey from Conception, a port of Chili, and from Iquiqui, another port, situate in the southern part of Peru. More than 60,000 quintals have been already brought there for sale.—(Ann. de Mines.)

X. *Quantity of Copper raised in Cornwall.*

In Six Months, ending June 30, 1821.

The produce of 64 mines, of which the following are the six principal ones, viz.

	Ore.	Copper.
Dolcoath	5679 tons	405 tons
United Mines	4199	394
Wheal Abraham, &c.	4522	330
Treskerby	2497	253
Consolidated Mines	2508	207
Pembroke	1698	136
	21103	1725
58 other mines	24927	2082
	46030	3807

Produce of the ore, 8¼ per cent.
Price of copper, 107*l.* 6*s.* per ton.

In Six Months ending Dec. 1821.

The produce of 74 mines, of which the following are the six principal ones, viz.

	Ore.	Copper.
Consolidated Mines	6089 tons	540 tons
United Mines	5191	487
Dolcoath	5557	395
Wheal Abraham	4417	342
Pembroke	2079	190
East Crinnis	1512	188
	24845	2142
68 other mines	27553	2565
	52398	4707

General Return of Copper raised in Great Britain and Ireland.

One Year ending June, 1821.

	Tons.	cwt.	qr.	lbs.
Cornwall	7764	15	1	11
Anglesea, about	500	0	0	0
Devon	476	0	0	0
Ireland, Wales, Staffordshire, Scotland, &c.	740	2	2	16
	9480	17	3	27

For similar statements for preceding periods, see *Annals of Philosophy,* vol. i. New Series, p. 394.

ARTICLE XVII.

NEW SCIENTIFIC BOOKS

JUST PUBLISHED.

A Practical Essay on the Strength of Cast Iron, and its Application in the Construction of Buildings and Machines. With new Experiments, Tables, &c. Illustrated by Four Engravings. By Thomas Tredgold, Civil Engineer. 8vo. 10*s.*

Lectures on the Elements of Botany. Part I. containing the Anatomy and Physiology of those Organs on which the Growth and Preservation of the Plant depend; with Explanations of the Terminology connected with these Parts. Illustrated by Marginal Cuts and Copper Plates. By Anthony Todd Thomson, FLS. MRCS. &c. 8vo.

A Case of Transverse Section of the Patella, in which perfect Osseous Union was procured; with Observations. By George Fielding, MRCS. &c. 1*s.*

A New and Classical Arrangement of the Bivalve Shells of the British Islands. By W. Turton, MD. 4to. Twenty Plates drawn and coloured from Original Specimens in the Author's Cabinet. 4*l.*

The Fossils of the South Downs, or Illustrations of the Geology of Sussex. By Gideon Mantell, FLS. MGS. FRCS. Royal 4to. With 42 Plates. 3*l.* 8*s.*

The Naturalist's Guide for collecting and preserving all Subjects of Natural History and Botany; intended for the Use of Students and Travellers. By W. Swainson, FRS. & FLS. 12mo. With Two Plates. 5*s.* 6*d.*

ARTICLE XVIII.

NEW PATENTS.

G. H. Palmer, Royal Mint, for improvements in the production of heat, by the application of well-known principles not hitherto made use of in the construction of furnaces of steam-engines and of air-furnaces in general, whereby a considerable saving of fuel is obtained, and the total consumption of smoke may be effected.—Feb. 12.

J. F. Smith, Esq. Dunston-hall, Chesterfield, for improvements in dressing of piece goods made from silk or worsted, or of both these materials.—Feb. 12.

S. Davis, Upper East Smithfield, for an improvement upon the lock for guns, &c, enabling the lock to be used upon the percussion principle, or with gunpowder, without charging the lock or hammer.—Feb. 12.

T. Brunton, Commercial-road, for improvements upon the anchor.—Feb. 12.

E. Peck, Liverpool, for machinery to be worked by water, applicable to the moving of mills, &c. or for forcing or pumping water. Communicated to him by R. Bulkley, a foreigner.—Feb. 22.

W. E. Cochrane, Esq. Somerset-street, Portman-square, for improvements in the construction of lamps, whereby they are rendered capable of burning concrete oils, animal fat, and other similar substances.—Feb. 23.

W. Prickle, Mark-lane, for improvements in machinery for cutting out irregular forms in wood, &c. Communicated to him by J. P. Boyd, of Boston, in America.—March 2.

J. Higgins, Esq. Fulham, for improvements upon the construction of carriages.—March 2.

C. Yardley, Camberwell; for manufacturing glue from bones, by means of steam.—March 2.

J. Thompson, Regent-street, Westminster, for an improvement in the method of preparing steel for the manufacture of springs for carriages. —March 2.

J. Ruthven, Edinburgh, for a new method of procuring mechanical power.—March 2.

G. Stratton, Hampstead-road, for an improved process of consuming smoke.—March 2.

J. Gladstone, Liverpool, for a chain of a new and improved construction.—March 12.

R. B. Bate, Poultry, for improvements upon hydrometers and saccharometers.—March 21.

W. E. E. Conwell, Ratcliff Highway, for an improvement in the preparation of a purgative vegetable oil.—March 21.

S. Robinson, Leeds, for improvements on a machine for shearing and cropping woollen cloth.—March 21.

G. Stephenson, Long Beaton, Northumberland, for improvements in steam-engines.—March 21.

R. S. Harford, Ebro Vale Ironworks, for an improvement in the heating processes in the manufacture of malleable iron.—March 21.

W. Church, Nelson-square, for an improved apparatus in printing. —March 21.

A. Clarke, Esq. Dron, Louchars, for an improvement in the boilers and condensers of steam engines.—March 21.

W. Pride, Uley, Gloucestershire, engineer, for a self-regulating apparatus for spooling and warping woollen, or other warps or chains. —April. 16.

W. Daniell, Abocarne, Monmouthshire, manufacturer of iron, for certain improvements in the rolling of iron into bars, used for manufacturing tin plates.—April 16.

B. Cook, Birmingham, patent tube manufacturer, for a certain mixture, or preparation, which may be used with advantage in preventing the damage of accident from fire.—April 16.

J. Grimshaw, Bishopwearmouth, Durham, ropemaker, for a method of stitching, lacing, or manufacturing, flat ropes by means of certain rotative machinery worked by a steam-engine.—April 16.

ARTICLE XIX.

METEOROLOGICAL TABLE.

1822.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
4th Mon.								
April 1	N	30·45	30·37	58	37	—	—	16
2	N E	30·43	30·37	55	33	—	—	20
3	N W	30·43	30·29	55	36	—		12
4	N W	30·29	30·17	56	44	—		5
5	N W	30·17	30·01	52	41	—		
6	N W	30·05	30·01	54	35	—		17
7	N	30·12	30·05	54	30	50		
8	N E	30·16	30·12	52	27	—	02	18
9	N E	30·15	30·13	51	28	—	—	15
10	N E	30·15	30·13	49	33	—	—	15
11	E	30·13	29·87	50	39	—	06	
12	E	29·98	29·86	55	40	—	35	5
13	S W	30·12	29·98	59	39	—	03	9
14	S E	30·12	30·08	65	48	56		
15	N E	30·08	30·06	66	48	—	1·02	9
16	N E	30·06	30·03	59	46	—	03	8
17	S E	30·03	29·78	58	45	—	—	9
18	N	29·81	29·78	56	39	—	15	6
19	Var.	29·81	29·81	58	38	—	18	9
20	S W	29·81	29·72	59	44	—	08	16
21	S E	29·72	29·49	62	47	—	18	
22	S	29·49	29·44	60	39	—	06	7
23	S W	29·70	29·40	58	41	—	03	4
24	S W	29·70	29·65	60	47	52	10	13
25	S W	29.93	29·65	62	41	—	02	11
26	S W	30·08	29·93	60	45	—	04	
27	S	30·31	30·08	63	51	—	08	3
28	S	30·31	30·30	68	42	—	01	
29	E	30·30	30·25	66	40	—		
30	E	30·36	30·30	68	39	35		17
		30·45	29·40	68	27	1·93	2·44	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Fourth Month.—1. Bleak. 2. A very cold wind all day: a lunar halo in the evening. 3. Fine: lunar corona. 4. Fine. 5. Fine. 6. Cloudy and fine. 7. Fine. 8. Fine, with occasional clouds: some hail about four, p. m. 9. White frost: fine. 10. Bleak: slight hail showers. 11. Very cold wind: cloudy. 12. Stormy. 13. Cloudy. 14. Fine. 15. Cloudy morning: rainy afternoon and night. 16. Showery and fine at intervals. 17. Cloudy. 18. Rain. 19. Gentle showers: hail in the evening. 20. Cloudy; fine. 21. Fine. 22. Cloudy morning: fine afternoon. 23. Showery. 24. Showery. 25. Showery. 26. Fine: windy. 27. Rain. 28, 29, 30. Fine.

RESULTS.

Winds: N, 3; NE, 6; E, 4; SE, 3; S, 3; SW, 6; NW, 4; Var. 1.

Barometer:	Mean height	
	For the month	30·022 inches.
	For the lunar period, ending the 14th	30·169
	For 12 days, ending the 1st (moon north)	30·146
	For 15 days, ending the 16th (moon south)	30·120
	For 12 days, ending the 28th (moon north)	29·822
Thermometer:	Mean height	
	For the month	49·166°
	For the lunar period	47·783
	For 30 days, the sun in Aries	47·870
Evaporation		1·93 in.
Rain		2·44

Laboratory, Stratford, Fifth Month, 25, 1822.

R. HOWARD.

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